AIR, FOOD AND EXERCISES.

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AN ESSAY

ON THE PREDISPOSING CAUSES OF DISEASE.

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PREFACE.

I HOPE that the chief proposition of this essay has been made so plain that I need say but little in the way of preface. A critic of the first edition said that it was written for the public, as distinguished from the medical profession. I thought I detected a certain (or uncertain) air of reproach in his tone, as if he meant distantly to insinuate a suggestion of unprofessionalism in the behaviour of a doctor who should write for the public. As a fact, the first edition was written, so far as I know, for the medical profession. It appeared as a series of articles in the *Scalpel,* a journal read only by the medical profession. No doubt after its separate publication, the public read the essay eagerly, the first edition being exhausted before a single medical criticism had appeared. The second edition was as rapidly cleared out; and no doubt another edition or two might have been disposed of. But I was not quite satisfied to go on with the work in the form which it had in 1896. In the original preface I had suggested that I might amplify my observations if the essay was received favourably. But in the intermediate

period between the publication of the second edition and any new one, I thought I ought to appeal to the medical profession directly and solely, before appealing to the public in a matter in which both the profession and the public are most deeply interested, and which seems to me indeed to be of the most vital importance to both. Accordingly I wrote in 1901 a book entitled "Aphorisms, Definitions, Reflections and Paradoxes: Medical, Surgical, and Dietetic." This being sent to various medical journals, elicited a few reviews, some depreciatory, and some highly laudatory; but on the whole the book fell flat, some critics even hinting that the language was too technical, a rather curious criticism of a book addressed and dedicated to a learned profession. This being so, I had to consider what I should do. It is rather a fine point to say what a medical man ought to do when he is not attached to a medical school, and yet feels that he has something very important to say on professional subjects. The direct appeal that I made to the medical profession fell on deaf ears. A few private letters of a more or less appreciative kind did not seem to me a sufficient answer to the question I addressed to my colleagues —How is it that when we know as physiologists that there is twice or thrice as much lymph in the body as there is of blood, no use whatever is made of this fact in medical practice? This overplus of lymph collected in the connective tissue of the body appears to me to be the basis on which the

great mass of disease is built. It appears to me to be of the very utmost consequence as regards the causation, the treatment, and the prevention of disease. And my colleagues having as a body ignored the question (although a few of them seem to have been greatly and effectively interested in it) I have felt myself compelled, in view of the shortness of life and of the probability that I cannot in the nature of things expect to have many more opportunities of addressing either my colleagues or the public, to appeal at once to as wide an audience as I can. This book is, then, written frankly for the public, in the hope that its perusal may simplify and clarify their ideas upon the subject of disease, and that, by having their attention directed to its chief cause, as I conceive it, they may be enabled to considerably increase the length of efficient and healthy life, as also to obtain some amount of relief from the calamities which, in spite of all the advances of medicine and surgery, they still are called upon to endure.

A. R.

Bradford,
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INTRODUCTION.

MEDICINE, connected, as the word is, with the Latin verb *mederi*, to heal, may be defined as the theory and art of managing the sick, of restoring them to health, and of preventing the occurrence or recurrence of illness. It is evident that the last branch of medicine named, that which is occupied with the prevention of illness, is one of late growth; and, as a historical fact, it has come into existence only of late years. I am not aware that the idea of preventing illness entered into the minds of the classic Greek and Latin writers on medicine at all. With cure, on the other hand, of diseases which had already arisen, they occupied themselves greatly, just as we do now. In the opinion of the writer of this essay, very little has been done in the domain of the prevention of illness and of disease, compared with what may be expected to be done in the future, and with what may be done now. Evidently, before preventive medicine can attain any great measure of success, we must know what the causes of disease and of illness are. To treat disease, it is not necessary to know much, or, in fact, almost anything of its causes. At least, long before it occurs to us to inquire as to its causes we adopt almost instinctively a method of treatment, and, if the disease is at all severe,

that method usually involves the three practical points of putting the patient at rest in bed, of giving him medicine in some form or other, and of altering his diet, since we seem to feel that we cannot feed him in sickness as he has been accustomed to be fed in health. Nature herself indeed points the way to us in this matter, for, if the patient is very ill, or even moderately so, he is naturally unable to be up, and his appetite is generally entirely absent, or at least much impaired. This is so at least in those cases of illness in which feverishness is present, that is, those in which the pulse is quickened and the temperature is elevated, as in fever, and inflammation, and severe injury. But if after a time it occurs to us that perhaps it might have been possible to avoid the fever, to prevent the inflammation, or to escape the injury, it is plain that we must first know what are the causes of these different conditions of body before we can offer advice as to how they may be prevented in the future either in the case of the given patient or of others. As to the last named condition, that of the injury, it will at once occur to every one that to avoid or to prevent a recurrence of that will offer the simplest case of the application of preventive medicine, since it is comparatively easy to see whether knife, or industrial tool, or warlike arms, or accidental fall have caused the condition; and prevention will resolve itself therefore into the one piece of advice to be more careful in the future. As to the fever and the inflammation, the problem

is much more complicated, and it is more complicated still as regards the occurrence of such a condition as tumour-growth ; but it is evident that the means recommended by the medical adviser, by the adoption of which these evils are to be escaped, will differ according to the view which he takes of their causation. In point of fact, as regards the incidence of fevers only, has much thought been expended on their causation, while even as to this opinions are divided into two great classes, one holding with the modern sanitarians that of all the causes of disease, and particularly of all the causes of fever, vitiation of air is far the most important, while the other holds, as Moses did at Kibroth Hattaava, that improper feeding is the cause of fevers, and that if the Israelites gathered and consumed quails in excess for two days and a night, it was no wonder that plague should break out among them, and that it should be necessary to bury at Kibroth Hattaava those of them that lusted. Evidently the advice calculated to prevent the outbreak of the pestilence that stalketh in darkness or of the destruction that wasteth at noon-day; the advice given as to how we are to prevent scarlet fever or typhoid or influenza, will differ according to the different views we may take as to the causation of these affections. On the former view much attention will be directed to the opening of windows so as to obtain the freest ventilation, and to supplying abundant cubic space to each person, so as to prevent overcrowding; on the latter

we shall devote much attention to the quantity and quality of the food (and drink) taken, and to the amount of time that should elapse between each meal.

As a preliminary to the consideration of the question whether it is more likely that bad vitiated air or wrong feeding is the more potent cause of the incidence of fever, let us consider which of these two great sets of physiological processes most alters a man. A man emits as much carbonic acid gas through his respiration as would suffice to supply about half a pound of carbon daily were it to be chemically separated from the carbonic acid gas. This is a large amount, and bears quite a considerable proportion to his body weight of say 140 lbs., or about ·7 per cent. But if he takes what is generally considered the quite moderate allowance of 2 lbs. of food daily, it is evident that he consumes his own weight of food in 70 days, or a little over two months; while if he takes 3 lbs. of food daily, the quantity recommended by Dr. King Chambers for the nursing mother, he consumes his own weight of food in 46 days. Some physiologists contemplate with equanimity the consumption of 5 lbs. weight of food daily, and at this rate a man would eat his own weight in 28 days. I hope to say something later regarding the very extensive quantities of fluid which in various forms pass into and out of the blood daily; but, referring now to food alone, as distinguished from water, I think I am justified in

saying that *a priori*, so to say, and on the general merits of the question, it is more likely that a man's body should be modified by food than by air. Of course there are many other changes effected by the respiration than the mere emission of half a pound of carbon daily. Other organic materials, not to mention a quite considerable amount of watery vapour, are given forth by the lungs, but the total amount of change in the body effected by the respiration is very much less than the amount effected by the digestion and assimilation of food and water in the body. Whoever, therefore, should argue that food is probably a greater agent in producing health or disease in the body than air, founds his opinion on substantial facts of this kind; and, at any rate, no inherent improbability attaches to his contention.

As to other affections of the body besides the fevers, the inflammations, for instance, and particularly as to the occurrence of tumour-growths, it will probably be admitted that scarcely anyone thinks of methods of preventing them, almost every one tending to look upon these conditions as part of the inevitable order of Nature. Let us hope that another view will project itself on our minds before we finish our consideration of the subject, so that some means of preventing these evils that now afflict humanity may arise before our imagination, and offer itself to our practical grasp. At present, however, I fear it must be admitted that any one who hints that perhaps more study of the causes of

the inflammations and tumour growths might enable us to suggest methods by which they might be prevented, and, especially if in order to this end, he suggests that it is necessary to exercise a certain amount of self-restraint, is apt to have his judgment questioned as that of a visionary.

It seems to be evident that medicine, although appealing to almost every human being with fascinating and even absorbing interest, must be a subject which presents many and great difficulties. The differences of opinion and view between different medical experts, regarding the subject matter with which they deal, offer one of the most striking proofs of this. Before any department of human affairs can be said to be understood, the facts concerning it must be clearly stated, they must be marshalled in a more or less complete manner, the law or laws of them, or, at least, their natural order declared, and their causes displayed. When these things have been done, and particularly when the prediction has been ventured on, *and verified,* as to when and in what circumstances the facts will arise or appear on the one hand; and, on the other, by what means their existence or onset may be prevented, then the phenomena may be said to have been scientifically or philosophically arranged, verified and declared. And when, or if, these things shall have been accomplished in Medicine, obviously there can be no differences of opinion or view between different medical experts; at least, no fundamental contradiction of view, although, of

course, there may be, and must be, as in all other branches of science, some or even considerable differences of opinion as to less or more. For in medicine, as in all other domains of knowledge, the phenomena dealt with, while they do not differ in kind, as between case and case, do differ very materially indeed in quantity or amount. The best theory is that which best harmonizes with and accounts for the facts. And it is interesting to observe how, as science has advanced in other departments of knowledge, better theories have supplanted worse ones, since, as knowledge of medicine advances, the same thing will no doubt be observed, one department of knowledge being governed by the same principles as obtain in other departments of inquiry into the constitution and course of nature. An illustration from the history of astronomy may perhaps be allowable here to shew the similarity of the lines of inquiry into the course of nature in all directions. The theory which seems so natural to a dweller on the earth that at first and for a long time no one would think of questioning it, that the earth is of very great size, that it is at the centre of the universe, and that the sun, moon, planets and fixed stars are revolving round it, does account for a number of astronomical facts; it is only when it is found that it does not account for or explain a large number of other facts, and that indeed it is quite incompatible with and opposed to them, that we are compelled to discard it in favour of one which supposes the sun

to be more or less fixed, if not in space, at least in relation to his own sphere of action, and that the planets and their satellites, our own small earth included, revolve around him. But just as, say, six passengers in one compartment of a railway carriage may change their places frequently within the limits of the compartment at the same time that the train may be carrying them all rapidly to their destination, so we recognise that the sun, with all his attendant planets and their satellites, may be moving onward, and that even rapidly, to some unknown point in space. The wider theory supplants the narrower one because it explains more of the facts. It may in turn be destined to be supplanted by one still wider if it shall be found that some astronomical facts are not explained by the theory; but in the meantime all astronomical experts accept the theory as far as it goes, and none are to be found who oppose it. The facts of medical science, however, are much more complicated and obscure than are those of astronomy (albeit to work out the complete inter-actions of even three bodies obeying the law of gravitation transcends, it is said, the highest powers of mathematics, how much more an indefinite number of such bodies?) at least they probably are so, since it is not uncommon to find medical experts differing fundamentally in their views as to the causation of certain conditions, and, therefore, opposing one another in their recommendations as to treatment. I refer particularly to the facts of depression or of

defect or deficiency of function, as, *e.g.*, lowering of the temperature of the body, slowing of the pulse, &c., of which some account will have to be given later. Some medical experts attribute these conditions to causes the exact opposite of those alleged by others to be the causes. In this particular, however, it must be said, the public are hard to please. If doctors agree, the public are apt to attribute their agreement to professional or class feeling; while, if they differ, they are apt to ask—who shall decide? The answer, however, when or if this unfortunate state of things occurs, is, I should like to say, just this. The public must decide; each man for himself must decide; reason and judgment must decide. The duty of the public, in a condition of opinion so unfortunate as this, is to receive the different views, to ask for the reasons given in support of each, to weigh them up as far as judgment and reason can do so, asking, if necessary, for help from other persons accustomed to weigh evidence in general, in order to be able to decide which of the opposing views is correct; and to act accordingly. Differences of opinion, after all, are not confined to medical experts. It is doubtful if they differ from one another as often or as widely as lawyers or politicians or statesmen or divines. Emotion sways and excites men in their efforts to settle differences in all of these directions; but nothing and no expert opinion can relieve the individual man from the need to exercise his judgment, his reason, and his conscience, in face of

contradictory advice tendered to him regarding the interests of those entrusted to his care, and whom he may feel to be dearer to him than his own life.

A very noticeable and noteworthy aspect of the difficulties, and even the chaotic disorder of the facts with which medicine has to deal, appears when we think of the widely different conditions with which she has to occupy herself. I mentioned fever, inflammation, tumour-growth and injury a little while ago as examples of different classes of disease. It will be necessary to offer definitions of these different conditions later, so as to separate them from one another, if it be possible to do so, and, therefore, to be able to say what we mean when we say that one person has a fever, and another is suffering from inflammation, as also to say how these states of body are to be prevented. But before we reach this attempt at definition, and of clearing up of confusion, we are met by the difficulty that the inflammations themselves are exceedingly numerous—they are co-extensive, in fact, with every organ and even with every tissue of the body—and that no reason appears at the outset why one person should suffer from inflammation of the throat or tonsillitis, as it is called, another from inflammation of the lung or pneumonia, while still another should have inflammation of the stomach or gastritis, and so on through the very numerous list of the inflammations. The explanation given by scientific medicine of the reason why these different conditions occur in different persons

or in the same persons at different times is not altogether satisfactory, albeit it is all that has been yet given, and apparently all that ever can be given to us. It is this. One person is said to have tonsillitis rather than pneumonia because the throat was the weaker place rather than the lung, while another has pneumonia rather than tonsillitis because the lung was weaker or had a lower resistance than the throat. It was the direction of his least resistance, as the expression is. But if we come to inquire why or how it is the direction of least resistance, it is not always possible to get or to give an answer. Very often it is said to be because of a previous occurrence of that affection in the body. A person is subject to recurring attacks of tonsillitis, each of which weakens the part and makes it more susceptible to subsequent attacks. The insufficiency of this explanation, however, appears when we reflect that we have to account also for the occurrence of the first attack. In this, as in all other attempts made by the human mind at scientific accuracy and explanation, we are very apt to become involved in a process of circular reasoning. Thus we say the tonsil was the direction of his least resistance; it was his weakest organ or place; and, therefore, he got tonsillitis. And we also say that the existence of the tonsillitis (rather than of pneumonia, for instance) is a proof that the tonsil was the direction of his least resistance. We account for the low (or least) resistance by the tonsillitis, and we account for the tonsillitis by the

low (or least) resistance. The same difficulty meets us in other directions in scientific inquiry. We say, for example (I confine myself to medical illustrations, although I might have found them anywhere and everywhere in other departments, as philosophy, theology, morals, sociology, &c.), a man, his father, and his grandfather, all suffered from bronchitis because the family were delicate, and we advance as the reason why we say the family is delicate, the fact of the occurrence of the bronchitis in three generations of the stock. The truth seems to be that scientific explanations, so called, cannot explain anything, and themselves in turn demand explanation. To say that a man had tonsillitis rather than pneumonia because the tonsil was the direction of his least resistance, is more high-sounding and pretentious than really explanatory, but it is all the explanation which, as a rule, it is competent for us to offer. As has been said, the difficulty is of the same kind as confronts us in other departments of knowledge. In the science of grammar, for instance, or of language, if we want to know the meaning of the word *acute*, and look it up in a dictionary, we shall find among other synonyms the word *sharp* given as an equivalent; and if we look up the word *sharp* we shall find among other synonyms the word *acute* as an equivalent. We can never get beyond our limitations in our attempted enquiries into nature, and it is hopeless to attempt to do so. A man cannot jump out of his own skin or get away from his own shadow in

sunlight. The brighter the sunlight the more noticeable the shadow. In the obscurity of night there is no shadow at all. If an acute critic suggests that in my attempt to clear up obscurity I am myself guilty of a similar course of circular reasoning in using such terms as synonym and equivalent, he may be correct; but indeed to some extent I fear it is impossible to avoid it.

The obscurity and complication of the facts with which she has to deal may, therefore, well be advanced as a reason why medicine has not yet made such an amount of scientific advance as will be necessary before she becomes, with her body of rules, formulated for the guidance of humanity to health, a light to the path and a lamp to the eyes. This obscurity is indeed further darkened by the fact that she deals with conditions and states of the body or organism rather than with the organism itself. Almost before the physician has formulated a definite conception of what he means by the organism or body, in health; before he has had time to do so, he finds himself attempting to understand various alterations or changes in its states, which alterations he terms disease, and which, as they are the subject matter of his art, he finds himself compelled to define for himself, and also to clearly expound to others. And, in order that he may be able to do this, it is plain that he must make a long and wide excursus into the wanderjahre of the collection of facts, before he can settle down into the lehrjahre of the methodised order and

arrangements of his facts. When or if, however, the physician can succeed in doing this, when he sees the order and the laws of his facts, the application of discovered laws to new facts must result in greater simplification of the facts, in the diminution of their obscurity, and in the conversion of difficulty, disorder and darkness into perspicuity, method, order and light. It is, it seems to me, through a study specially of the blood and lymph, through examination of the mode of their formation, of their uses and functions, and of their conveyance to and their circulation in every one of the tissues of the body, that this order and methodised arrangement are to be introduced. By this means the chaos of the incidence of disease in the body is to be reduced to order, and the seeming impossibility of understanding how one person should have sore throat, another headache, another bronchitis, another pneumonia, another rheumatism, another inflammation of the heart, and so on through a catalogue of diseases that seems interminable—the same person may at different times of life suffer from any or all of these affections, and from a large number more—it is through a study of the circulation of the blood and lymph that these disorders are to be understood and the laws of their incidence seen. For, truly, as soon as it is perceived that the same blood goes everywhere in the body, and when it is seen that if the blood is properly and healthily made, it will nourish properly every organ and tissue of the body, while, if it is not healthily or properly made,

it cannot do so, and must, therefore, instead of carrying health to them, convey disease; and when, lastly, a little observation and reflection have been given to the mode of formation and repair of the blood, then it is that chaos is replaced by order, confusion by simplicity, and darkness by light.

Medicine has, however, already made much scientific advance. In the whole of anatomy her method is scientific, and in much of physiology, pathology, and the principles of therapeutics, though scarcely in pharmacology. The methods are also scientific in subsidiary domains like chemistry. Bacon's dictum regarding medicine that it has been *magis elaborata quam amplificata*, rather laboured than advanced, is, if it cannot be said to be totally untrue, at least much less true than when he said it. But there is one other great reason for medicine's failure to make so great an advance into the scientific arena as we might have expected, a reason which she shares with some other departments of enquiry, and which is apt equally to complicate their investigation, viz., what has been called the difficulty of the personal equation. Phenomena which ought to appear the same seem often to differ very much indeed when manifested by this, that or the other person or organism, just as in sociology or in politics phenomena are often very different when manifested by this, that or the other personality, or by this, that or the other different nationality. Physiological and medical phenomena are not like mathematical facts, in which we can

discard variables like the widths of lines, and the magnitudes of points, without seriously vitiating our conclusions. In medicine, the analagous variables have much more weight and influence, and if we attempt to discard them, as in mathematics we may the width of our lines and the magnitude of our points, we shall find our conclusions seriously vitiated and interfered with.

There is even a special form of the personal equation which is more apt to disturb the judgment and the equanimity of the medical man than that of the representative of almost any other calling, unless indeed it be the pastor and preacher, and that is the unwillingness manifested by the average human being to seriously consider any advice which involves the giving up of any personal gratification or which contemplates in any form the idea of self-restraint or self-government. Now it is very frequently the case that the medical man is called upon to point out that some form of self-indulgence is the cause of the ailments, and particularly of the recurring ailments, from which patients suffer. In this case he feels it to be his duty to say what he has to say, but is afraid how it will be received, and often indeed feels that saying it may lead to a breach between himself and his client, a contingency which he is on all grounds anxious to avoid. It will be felt perhaps how very delicate the position becomes if, for instance, a mother is not excercising a wise government over her children's food-habits, and if

in consequence the children's health is suffering. It might be thought perhaps that mothers would be only too happy to know what would prevent their children's illnesses, and would, therefore, be only too glad to do anything to obviate them. But a great many considerations arise to prevent their taking such a course, mostly of a social nature, and coming through channels which it is very difficult to control. And, of course, at bottom there is great dislike to imposing limits either to our own gratification or to that of our children. Then again, other exponents of medical views may not have such strong opinions, and for these and many other reasons which perhaps are as well unexpressed, the happy relations which it is so desirable, if possible, to maintain between adviser and client are apt to come to an end; and this however tactfully the unpleasant advice has been offered, and however well the unpalatable pill has been gilded. Perhaps these difficulties are as great in other walks of life. Each person gets to know them in his own department. It does not seem possible to go through life very smoothly, however careful one may be to avoid causes of offence. Still each must judge for himself how far he ought to go in attempting to hold the balance even between a tactful desire to please and a compulsion laid on him to interpret the facts and the law of the facts truly as far as in him lies.

The variabilities in the phenomena with which medicine has to occupy herself, the obscurities and the indefiniteness of them become greater rather

than less when we come to consider more closely what medicine is. Medicine is a science, or it may come to be a science of conditions, of the conditions of the human body in disease. An extension of the domain to cover the field of *animal* medicine, and, still further, to cover the field of *organic* medicine, that is, the medicine of organic or organised things (animal and plant), would very greatly widen its scope. It would, that is, greatly widen it as regards *details*, though as to *principles* this might be doubted. However, with this we need not at present concern ourselves. It is sufficiently difficult and confusing to deal with the disease-conditions of the human body, as these pass by gradations almost insensible from conditions of health to those of disease. Disease being any and every departure from health, the question at once is forced on us for consideration—What is health? Can we define health? There have been many definitions or descriptions of health, none of them theoretically perfect, yet practically of good service in clearing the mind of confusion. If, for example, we say that health is the normal, easy and painless balance between the organism and its environment, and that disease is any departure therefrom, we have a practical definition, though by no means a theoretically good one, of health and disease respectively; since the question at once arises what a normal balance is, and the settlement of this and similar questicns really involves the definition of what health is, so that we find

ourselves unconsciously begging the question and moving in a circle.

The reader will think of Mr. Herbert Spencer's definition of life, that it is "the continuous adjustment of internal relations to external relations," and will ask if this statement is a simplification of his ideas of life, or otherwise. What we want in a definition is something that will make the idea or the thing easier, simpler than it was before. If, we say, trying to get a practical working definition that disease is any and every condition in which the doctor is sent for, we are forced at once to reflect that the doctor is not always summoned when disease sets in, and also that persons are not always ill when they summon the doctor, since they may want a certificate that they are well, or a statement from him that they are fit to join the army or navy, or, for example, that they are safely insurable, or that they are able to perform this, that or the other piece of work. The truth is, it is almost impossible to give a definition of health, and, therefore, of disease, which is any departure therefrom, without introducing by paraphrase, if not explicitly, the very word or idea which we wish to define. To say, for instance, that health is organisation in normal, easy and painless action, sounds well until we ask what is "normal action," and when we attempt to answer this question we find that normal action and health is much the same thing, so that our high sounding definition seems in the end to come to the statement that health is health,

which, though undoubtedly true, is by no means instructive. If instead of a definition we attempt a description of health, and say that an organism is healthy when the pulse is not below say 60 a minute, nor above 90, when the temperature stands at about 98·4° F. or 37° C., when the respiration varies from 14 to 20 a minute, when the bowels act once a day, and 40 to 60 ounces of water are passed daily by the kidneys, all these functions being easily and painlessly performed; and so on, through all the functions of the body (and, it might have been added, of the mind also), we soon realise that the idea of health, simple as we thought it on commencing our study, is a very complicated affair indeed, and that our ideas concerning it, instead of becoming more and more simple, become, if not more and more obscure, yet at least more and more extended. Without, therefore, assuming more for our definition of health than it is reasonable to claim for it, and without attaching too much importance to our attempts at description, since our limits of variation may easily be exceeded without departure from health, we must leave attempts at definition and description and pass on to the consideration of the subject of health and its co-relative disease.

CHAPTER I.

The Problem stated. The present state of Medicine and Surgery, and their progress in the past.

IN current literature, professional and lay, we are continually being told of the progress of medicine and surgery. I shall have to say something from time to time in the course of these observations regarding the latter, although my remarks are mainly confined to medicine. I think there is too much surgery performed in England and the world at present. There can be no doubt that many diseased conditions, which it is now the custom, and which it has been increasingly the custom for some time past, to treat surgically, might be, and much more satisfactorily, treated medically, that is, by medical, and particularly by dietetic management. The cry of "operate, operate," is becoming too loud, and people are stretching out in too marked a manner mute hands appealing to surgery for help, and are tending to expect too much from surgical operations. When a surgeon takes upon himself to say that a natural structure is "obsolete and out of date," suggesting, by implication at least, that all might yet be well if

he were called in to remove it by surgical operation, his mental attitude, as well as the language in which he expresses it, strikes us as with a jar. And when he goes on to say "it is safe to predict that in the intestine of the man of the future there will be no such structure (as the vermiform appendix) found hanging from the caecum," one is inclined to say, it is unwise to prophesy unless you know. But if one is going to venture on prophecy, it is certainly clever to predict that something will occur or will not occur at a period of time so remote that neither the prophet nor any of his hearers will be here to see the prophecy verified, or to be disturbed if it is not verified. The prophet who predicted the end of the present dispensation in 20 years gave his auditors a much better chance. They could verify the truth or otherwise of the prediction. But although this particular prophecy cannot be verified in any reasonable time, a simple sum in proportion rises to the mind on reading rash and arrogant statements of this kind, viz., this: if 5,000 years (or 50,000 or more years, for it seems not impossible that men may have been on this planet for even that length of time) have not sufficed to abolish the vermiform appendix of the caecum, what likelihood is there that it will evolve to nothing in 1000 years hence, or 2000 or 3000 years? It does seem a pity that men so able and so knowing had not been entrusted with the making of man and of the world on which he was to dwell. They could have done the work so much

better. And what a number of useful hints have been lost because they were not consulted when the thankless task of fashioning them was in course of being accomplished. A structure rich in lymphoid elements has probably a useful part to play in the economy, and it would be far more useful to poor and suffering humanity, though possibly less immediately beneficial to the prophets, to advise it how to keep the appendix vermiformis and other parts sound and healthy than to proceed to remove them in the way that is now so freely proposed and carried out. Even after the appendix is removed, the *caecum or other parts of the intestine may become inflamed, and the appendix-less patient may still suffer from inflammation of the caecum or of the bowel, from peri-typhlitis or enteritis. Are we to excise the caecum in order to prevent typhlitis or peri-typhlitis? or the bowel, in order to prevent enteritis? Or would it be wise to amputate the head in order to prevent neuralgia of the face?

Obviously, if the causes which lead to the formation of some growth or abnormality in the body, or to some local inflammation in it, are allowed to go on acting after the inflammation has subsided, or after the growth or abnormality has been removed, either a recurrence of the growth or

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*Note.—Caecum is the Latin for blind, the Greek equivalent being τυΦλὸν. The name is given to that part of the bowel where the large intestine commences as a *cul de sac* or blind part, that is, with no passage through at that place. The appendix vermiformis springs from this part.

abnormality, or, when that is not possible, some other abnormality or diseased condition must be expected to occur in the body. Many instances of this have come under my observation, as they must have done under the observation of every experienced practitioner. A patient who gets sore throat, for example, or bronchitis, may, and not infrequently does, get another and another attack of the same affection, if, when the inflammation has been subdued by treatment, nothing is said to him as to the causes which led to his successive attacks. The well-known fact that the removal of a simple ovarian tumour is frequently followed some years afterwards by the formation of cancer about the stump of the simple tumour, is a striking corroboration of the truth of this view, as it also suggests to our minds that the causes of simple tumour and of malignant or cancerous tumour are probably the same, and that the long continuance of the action of the causes of simple tumour-growth leads in time to the production of cancerous growth. In any case, every experienced surgeon must have heard from time to time the statement—"I am much disappointed with the result of my operation." Surgery has its sphere, no doubt, and a very great one, but it seems at present to be overdone. But I do not wish to enter into any considerations of this sort. I do not deny, I rather re-echo, the assertion that, where operations are necessary, they are much safer in the performance and much more curative in their results than they

used to be. Nevertheless, and notwithstanding the brilliancy and striking character from time to time of the results of surgery, it has not done much for the lengthening of the life of the people. Compared with the results achieved by medicine, meaning by that name not only pharmaceutical but also sanitary and hygienic measures, the results of surgery have been in the aggregate very small indeed, As to medicine, however, what has been the result? In the year 1838 the Registrar-General began to compile his figures for England and Wales, and in the years 1838-40 the death-rate for the country was 22·33 per 1000 per annum. In the years 1898-1900 (60 years after) the death-rate averaged 18 per 1000 per annum, a fall of about 20 per cent. It would be a crude inference from these figures to assume, dividing 1000 by the ratio per thousand per annum, that the average duration of life for these sets of years had been forty-four and three-quarter years for the first period, and no less than fifty-five and a half years for the second. Nevertheless, the expectation of life *is* now considerably greater in England and Wales than it was when the Registrar-General began to compile his figures. A very interesting set of tables has been published in the supplement to the forty-fifth annual report of that official, from which it will be advisable to extract a few facts. It would take too long a time and occupy too much space to deal completely with the information contained in this and previous reports; but it appears if we compare

the expectation of life at birth at different periods of time in England, that a very considerable improvement has been effected. Thus in the 16 years, 1838-54, the expectation of life at birth for males was 39·91 years. In the 10 years, 1871-80, however, it had risen to 41·35 years, and in 1881-90 it reached 43·66 years. For females the corresponding expectations were :—For 1838-54, 41·85 years; for 1871-80, 44·62 years; and for 1881-90, no less than 47·18 years. If we were to assume, which is not the case,* that the numbers of males and females were equal, this would mean an increase in 1881-90 over 1838-54, to the average duration of life, of about four and a half years. If this number were multiplied by 32¼ millions, the estimated population of England and Wales in 1900 (supposing the improvement still to hold), no less a sum would be reached than 145,125,000 years of life as added to the life-time of the generation now living in England and Wales, as compared with the conditions obtaining in 1838-54. For the United Kingdom the corresponding addition would be no less than 180,000,000 years of life. Numbers like this strike the imagination with some of the surprise excited by the figures dealt with in astronomical enquiries; but so far as can be seen, they seem fairly to represent the gross increase, medical and surgical, made in the last 60 years in the life duration of the people of this country. If similar gains have been

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*Note.—In 1881-90, out of a million children born, 485,527 were males, and 514,473 females.

effected (as to some extent they have) in other countries, the life gains to humanity, made by a diminished mortality, must be very great indeed. Beside numbers like these, even the addition of 30,000 years of life, which a great ovariotomist recently claimed that he had made through his own personal instrumentality to the life-time of his day and generation (great as it undoubtedly is as the influence of one man, now happily imitated by many others), pales into insignificance. It illustrates the different figures dealt with by medicine and surgery respectively. When the surgeon speaks of adding thousands of years of life to his generation, the physician (as represented by hygiene and sanitation) speaks of millions. The millions no doubt include the thousands, but the total result goes to show that, great as the influence of improved surgery has been, it has been comparatively small as compared with that of improved medicine, speaking of it in its widest sense. And further, sanitation is without drawback, which surgery is not. For, in considering the improvement effected by the latter, we are compelled to remember that at least some lives have been shortened where surgical operations have ended fatally; since it cannot be contended that all the lives cut short by surgical operations would have been terminated so soon had the patients been left alone. It will never be known to what figures such losses would amount; but whatever they are, they ought to be deducted from the gross addition made to human life by surgery. On the

other hand, all that is added to human life by sanitation is pure gain — we must hold with Socrates that ὁ βίος βιωτὸς, "life is worth living" —for I think I need not now and here discuss the question, already settled, whether, in saving the weaklings, sanitation can in any sense be said to have deteriorated the race. Assuming that as settled, the question remains in what forms the gains have been effected. I hope to consider the increase in the duration of life at different age periods later. In the meantime, it may be said that the gains on the large scale in the last 50 or 60 years have been mainly two. We have very much diminished the mortality from the fevers, or what are known as the zymotic diseases, and we have very much diminished the mortality from consumption. In the five years, 1850-4, the mortality from the fevers amounted to 5234 per million per annum on the average. In the five years, 1875-9, it had fallen to 3911 per million per annum. In 1871-80 the average mortality from the zymotic diseases in England and Wales was at the rate of 3724 per million per annum. In the five years, 1881-5, it averaged 2797; in 1886-90 it was 2506; in the decennium 1881-90 it averaged 2656. In 1891-5 it was 2747. The average of 1889 to 1898 was at the rate of 2663 per million per annum, the rate in 1897 being 2811. The rate has been rather upwards in the last few years. The decennium, 1889-1898, has shewn no less mortality from this cause than the decennium ending in 1890;

it is, in fact, rather higher if anything; but the rate in the decennium ending 1898 has been less than that of the five years ending 1854 by no less than about one-half, or 49 per cent. Why this is, is an interesting question, which I propose to discuss later.

In the case of consumption, the mortality in the five years, 1850-54, was at the rate of 2811 per million per annum. This had fallen to 2130 per million per annum in the five years 1875-9. In 1881-5 the mortality was at the rate of 1830 per million per annum; in 1886-90, it was 1635; in 1891-5, it was 1461; and in 1896-1900 it was 1321 per million per annum; so that the fall in the mortality from consumption in England and Wales since 1850-4 has been no less than 53 per cent., or considerably over one half. And both in the case of the fevers and in that of consumption the gain to the health of the community must be considered most satisfactory, more so perhaps in the case of consumption than in that of the fevers, since the fall in consumption appears to be still steadily progressing, while that from fevers has been somewhat checked. It may also be noted in passing as regards consumption, that the diminution in its mortality had begun steadily to progress long before any special crusade against it had been instituted, and that, good as that crusade is, and laudable in its attempted results, we must be careful of attributing to it effects beyond those to which it may be fairly entitled.

Besides these gains to the health of the people, known to have accrued during the registration period, there are others of a very satisfactory character. The following table, copied from the report of the Registrar-General for England and Wales for 1900, will shew, for those who are interested in such inquiries, the diminution which has been occurring in the mortality of the people since 1838, when the Registrar.General first began to compile his figures :—

ENGLAND AND WALES.—Annual Death-rates at Twelve Age-periods in Groups of Years, 1838–1900.—Males and Females.

PERIOD.	AGES.												
	ALL AGES	0—	5—	10—	15—	20—	25—	35—	45—	55—	65—	75—	85 and upwards.
	DEATHS TO 1000 LIVING.												
	MALES.												
1838 to 1900	**22·1**	**68·3**	**7·1**	**4·0**	**5·6**	**7·6**	**9·0**	**12·8**	**19·0**	**33·5**	**68·1**	**147·6**	**308·8**
1841–50	23·1	71·2	9·2	5·1	7·1	9·5	9·9	12·9	18·2	31·8	67·5	143·3	312·3
1851–60	23·1	72·7	8·5	4·9	6·7	8·8	9·6	12·5	18·0	31·0	65·5	146·7	308·2
1861–70	23·7	73·5	8·2	4·5	6·2	8·5	9·9	13·5	19·2	33·1	67·1	147·2	315·0
1871–80	22·7	68·5	6·7	3·7	5·3	7·4	9·4	13·8	20·1	34·9	69·7	150·8	327·4
1881–90	20·3	61·6	5·4	3·0	4·3	5·7	7·8	12·4	19·4	34·7	70·4	146·6	305·8
1891–1900	19·3	60·8	4·2	2·4	3·8	5·2	7·0	11·9	19·4	35·8	69·0	147·0	285·1
1841–45	22·2	68·7	8·8	4·8	6·8	9·0	9·4	12·2	17·2	30·3	65·5	143·7	305·1
1846–50	24·1	7·38	9·5	5·4	7·3	10·0	10·5	13·6	19·2	33·2	69·5	153·0	319·5
1851–55	23·5	73·9	8·8	5·1	7·0	9·2	10·0	12·9	18·6	31·5	66·6	150·8	311‘0
1856–60	22·6	71·5	8·3	4·6	6·4	8·4	9·2	12·2	17·4	30·4	64·3	142·6	305·4
1861–65	23·7	74·0	8·5	4·7	6·4	8·7	9·7	13.2	18·9	32·8	66·3	145·8	316·4
1866–70	23·7	72·9	7·9	4·3	6·0	8·3	10 1	13·8	19·6	33·5	67·8	148·7	313·6
1871–75	23·3	69·9	7·1	4·0	5·7	8·1	10·0	14·3	20·3	34·8	70·0	149·5	323·3
1876–80	22·1	67·0	6·3	3·4	4·9	6·7	8·7	13·4	19·8	34·9	69·4	152·2	331·6
1881–85	20·5	61·3	5·8	3·2	4·5	6·0	8·2	12·8	19·3	34·2	68·7	145·4	297·8
1886–90	20·0	61·9	4·9	2·8	4·1	5·5	7·4	12·0	19·4	35·2	72·1	147·9	313·8
1891–95	19·8	62·0	4·5	2·5	4·0	5·3	7·2	12·1	19·8	36·3	71·8	149·7	290·1
1896–1900	18·8	59·6	3·8	2·2	3·5	5·1	6·7	11·7	19·0	35·3	66·2	144·3	280·2
	FEMALES.												
1838 to 1900	**20·0**	**58·4**	**6·9**	**4·1**	**6·0**	**7·2**	**8·8**	**11·6**	**15·5**	**28·2**	**59·9**	**133·9**	**282·1**
1841–50	21·6	61·1	8·9	5·4	7·9	9·1	10·6	12·9	16·1	28·4	60·9	135·9	293·3
1851–60	21·4	63·0	8·4	5·1	7·4	8·6	10·0	12·2	15·3	27·1	58·9	134·5	288·9
1861–70	21·4	63·7	7·8	4·5	6·7	8·0	9·7	12·1	15·6	27·9	59·1	134·9	285·1
1871–80	20·1	58·4	6·3	3·7	5·5	6·8	8.6	11·6	15·6	28·7	61·0	135·4	296·4
1881–90	18·1	52·0	5·3	3·1	4·4	5·5	7 4	10·6	15·1	28·5	60·4	130·6	270·8
1891–1900	17·1	50·9	4·2	3·5	3·7	4·6	6·3	9·9	14·9	29·0	60·0	133·5	259·6
1841–45	20·6	58·6	8·6	5·2	7·7	8·6	9·9	12·2	15·1	27·2	59·1	131·8	288·6
1846–50	22·6	63·7	9·2	5·7	8·1	9·6	11·2	13·7	17·0	29·7	62·8	140·1	297·9
1851–55	21·8	63·8	8·5	5·3	7·8	8·9	10·3	12·6	15·8	27·8	59·6	137·1	292·0
1856–60	21·0	62·3	8·3	4·8	7·1	8·2	9·6	11·9	14·8	26·5	58·1	131·9	285·8
1861–65	21·5	64·1	8·3	4·8	6·9	8·2	9·8	12·1	15·5	27·9	59·1	133·7	287·7
1866–70	21·2	63·3	7·4	4·3	6·4	7·8	9·6	12·1	15·8	27·9	59·1	136·1	282·4
1871–75	20·7	60·0	6·6	4·0	5·9	7·4	9·2	12·0	15·9	28·7	61·2	135·3	293·8
1876–80	19·5	56·8	5·9	3·5	5·0	6·2	8·0	11·2	15·4	28·6	60·8	135‘4	299·0
1881–85	18·3	51·9	5·7	3·5	4·7	5·9	7·9	11·0	15·2	28·1	59·0	128·9	265·4
1866–90	17·8	52·0	4·9	2·9	4·1	5·2	6·9	10·3	15·0	28·8	61·7	132·3	276·2
1891–95	17·7	51·9	4·5	2·7	4·0	4·9	6·7	10·3	15·3	29·8	62·7	135·8	263·2
1896–1900	16·6	49·9	3·9	2·3	3·3	4·3	5·9	9·6	14·5	28·1	57·4	131·2	255·9

The analysis of these figures in detail would probably weary and confuse most readers, who will from personal examination of the table find much to interest and instruct, and that in a better way than any analyst can do it for them. But the following few observations may be made. First as regards females, with the exception of the age periods 55-65 and 65-75, the mortality is less in 1891-1900 than it was on the average of the years 1838-1900, the whole period for which the figures have been kept. Thus we find that the general mortality at all ages had fallen from 20 per 1000 per annum to 17·1 per 1000 in 1891-1900. From 0-5 years it had fallen from 58·4 per 1000 to 50·9. From 5-10 years of age it fell from 6·9 to 4·2. From 10-15 years of age it fell from 4·1 to 2·5. From 15-20 years of age it fell from 6·0 to 3·7. From 20-25 years of age it fell from 7·2 to 4·6. From 25-35 years of age it fell from 8·2 to 6·3. From 35-45 years of age it fell from 11·6 to 9·9. From 45-55 years of age it fell from 15·5 to 14·9. From 75-85 years of age it fell from 133·9 to 133·5. And from 85 and upwards it fell from 282·1, the average mortality over the whole period, to 259·6 in 1891-1900. In the age-periods alone 55-65 and 65-75 did the mortality rise, being in 1838-1900 28·2 per 1000 from 55-65, and 29 for the ten years ending in 1900, while from 65-75 the figures were for 1838-1900 59·9, and in 1891-1900, 60 per 1000 per annum. This means that with these exceptions the average expectation of life has been raised for

the whole of the female part of the population of England during that long period of time ; and this is certainly a very gratifyiug and satisfactory state of things, and one for which sanitation and medical progress may reasonably take credit. Among males the same general improvement has taken place, but with differences not quite so satisfactory as regards results. We find, for example, increased mortality among males for the three age-groups, 45-55, 55-65, and 65-75, in the later and shorter period, as compared with the average of the longer one. The reader can see for himself that at age-period 45-55 the average mortality of males for 1838-1900 was 19 per 1000 per annum. For 1891-1900 it was 19·4 per 1000 per annum. At age-period 55-65 the mortality was at the rate of 33·5 per 1000 per annum for 1838-1900, and 35·8 for 1891-1900; and at age-period 65-75 it was 68·1 for 1838-1900, as compared with 69 for 1891-1900. On the whole, however, both for males and females, the improvement has been very satisfactory and gratifying. Some other interesting facts arise to the student of these figures. At every age-period nearly, the mortality of males is greater than that of females at the corresponding age-periods. Thus from 0-5 years of age, from 5-10 years, and at every age-period except from 10-15 and from 15-20 years of age, the mortality of males is greater than that of females. At 10-15 years of age, however, and also at 15-20 years, the mortality of females living at those ages is greater than it is for males. Thus for

1838-1900 the male mortality was 4 per 1000 from 10-15 years, and 5·6 per 1000 from 15-20 years of age, while the corresponding female mortality was 4·1 per 1000 and 6 per 1000. For the ten years, 1891-1900, the same holds true for the age-period 10-15, for which the male mortality was 2·4 per 1000 and the female 2·5 per 1000 ; but it does not hold good for age-period 15-20 years, for the male mortality was at the rate of 3·8 per 1000 per annum, while the female was 3·7. The explanation of these disparities raises very interesting questions, but has not been wholly effected. Certain broad facts rise before the student of these figures. While, for instance, except at the age-periods already mentioned, the expectation of life is on the whole better in 1891-1900 than it was either on the average of 1838-1900 or in the ten years 1841-50, the improvement is much more noticeable in the early life-periods than it has been either in old age or in the middle or working periods of life. In the case of males from 25 to 45 years there has been some improvement in the expectation of life, which means probably also greater vigour at these ages to do the work of life, but, even at the best, the improvement has not been very great. From 45-75 the mortality is really a little greater in 1891-1900 than it was either over the average of the years 1838-1900, or than it was in the years 1841-50. Only from 75 onwards, when the work of life is over, has the expectation of life at the more advanced ages been increased. We ought to be

thankful for the improvement which has been effected, but, when it is all summed up, it is not yet so great as we had hoped, or as perhaps the prevailing optimism in the literature of the subject had led us to expect. The net result appears to be, in the case of males, that while in very early life, when work is yet in promise rather than performance, the expectation of life has been very considerably increased; that while in extreme old age, when the work of life is over, and when those who have lived well and have been fortunate may sit down to enjoy the fruits of their labours, the expectation of life has also been increased; in the middle or active periods of life, when the burden and heat of the day have to be borne, either the expectation of life has been only very little increased (ages 25-45), or it has been even somewhat diminished (45-75). Similar conclusions arise from a study of the results when lustra or quinquennia rather than decades are compared with one another, and with the average results extending over the whole period from 1838-1900. The reader will do well to make these comparisons for himself. Nothing can well afford a better bird's eye view, so to call it, of the changes (not all progressive, unfortunately, as regards the public interest) and improvements which have taken place in the public health since 1838, than the careful and prolonged study of such a table. We are at a great advantage as compared with our predecessors in the possession of such figures, since it makes our information, if

not accurate, at least much more approximately accurate than any which our ancestors possessed. They were obliged to trust to general impressions, while we can adduce comparative figures; and an intelligent attempt to realise on the part of each for himself what has been going on, coupled with the appreciation of the fact that the risks of males from overwork, accidents, anxiety, and the competition and struggle of life generally, are considerably greater than they are among females (who yet have their own special risks and troubles, no doubt), will do far more to enable the student to understand the general question of the improvement or otherwise in life than any other method of study.

If now we come to inquire how it is that these gains have been effected, the answer is obvious. These changes and improvements are the result of the sanitary efforts of the present and of the last generation. We have expended vast sums in improving (sometimes even in creating) drainage and sewage, in abolishing cellar-dwellings, diminishing over-crowding, opening up and widening narrow streets, opening public parks and recreation grounds, and generally in efforts to purify the air and abate nuisances; and we have obtained vast benefits as measured by the figures just stated. Following the statement of a great sanitarian, we have believed and have shaped our general national hygienic policy on the statement that "Statistical inquiries prove beyond a doubt that of the causes of death which are usually in action, impurity of

the air is the most important." As will abundantly appear in the sequel, this statement seems to me to be far too sweeping. It appears to me, indeed, quite impossible to accept it. Vitiation of air is no doubt a very important cause of illness and of death, but it is not by any means the most important, being second, and a long way second, to errors of diet. Whatever be its rank, however, as a cause affecting health, there can be no doubt that attention to the improvement of the air breathed has had a very great influence on the public health. Generally speaking, the effects having been mainly two, viz., the diminution of the incidence and of the mortality from the fevers and those from consumption, the general effect has been very much to benefit life at the earlier ages, and to diminish sickness and mortality during childhood and youth, since the effects of the zymotic diseases and of consumption are mainly felt at early ages, the large majority of these diseases occurring under 25 years of age.

There have been, however, at work on the health of the community other contributory causes besides those making for the improvement of the respired air. By failing to realise the importance of these, we are apt, erroneously, to attribute to sanitation effects due to other causes, so exaggerating the importance of sanitation, which, however, requires no fictitious aids to add to its consequence. Among these is the diminution which has taken place, and which appears to be still going on, in the birth-rate.

The birth-rate rises and falls under the operation of various causes connected with the prosperity or otherwise of the people ; but, speaking broadly, the birth-rate of England and Wales was at about 34·2 or 34·3 per 1000 per annum 50 years ago, and is under 29 per 1000 now, a fall of about 14 or 15 per cent. This is no isolated fact affecting the United Kingdom or England and Wales only. The table inserted on p. 41 from the report of the Registrar-General for England and Wales for 1900 shews that the diminution of the birth-rate is going on in all European countries without exception, even Norway and Sweden shewing the fall, although not to the same extent as is seen elsewhere. This is a fact of the utmost significance, and must, I think, be considered as coincident with great economic and moral changes in the character of peoples. We in this country have been accustomed to point to a low birth-rate as a mark of a people unexpansive, not given to colonisation, and somewhat contracted in their methods of behaviour and intellectual outlook. It is not necessary to offer any opinion on such views. Very much may be said in defence, as in criticism, of a high birth-rate on the one hand and of a low one on the other. The physician and hygienist is happily not called upon to defend or blame, it being sufficient if he notes the changes which are occurring among the peoples whose health he is considering, and their influence upon it. The noteworthy fact is that the process of lowering the birth-rate is general. In France, it will be observed,

the birth-rate was in 1900 actually lower than the death-rate, a fact which obtains in no other European country, and is not even approximated to in any except Ireland, whose latest returns make the birth-rate 22·7 per 1000 and the death-rate 19·6. It is impossible, I think, to suppose that a tendency which is seen in every European country is not of grave moral significance, and I shall dismiss this aspect of the question with one observation. If a diminishing birth-rate is, as we imagine it to be, a mark of decadence, or at least of want of progressiveness, as we continually assume in comparing our own country with France, for example, it is to be noted that we are persistently approximating to the French custom in this particular. So are all European countries. In America, we are told—but I do not know what the truth may be in this matter — that but for foreign immigration the numbers of the people would not be kept up, let alone increased, by the excess of births over deaths. It would probably be a very narrow view to assume that these simultaneous changes are an imitation of the French system. It is far more likely that the causes are much more general than that, and that they are to be found in changes in the economic and moral conditions of the various peoples concerned. From this (probably the correct) point of view, the diminution in the birth-rates in the various civilised countries ought rather to be viewed, like the sequence of day on night, as concomitant or successive effects of a common cause

or common causes, than as causes and effects of one another. It is our business to note what are the effects on the health of nations of a process which is so general and so wide-spread, that there is not a single European country in which a considerable fall in the birth-rate has not occurred in the last 25 years.

Births and Deaths per 1000 of the Population, in the United Kingdom and in Other European States.

(Tabulated from returns furnished by the authorities of the various States.)

Year.	United Kingdom.	England and Wales.	Scotland.	Ireland.	Denmark.	Norway.	Sweden.	Austria.	Hungary.	Switzerland.	German Empire.	Prussia.	The Netherlands.	Belgium.	France.	Spain.	Italy.
	Births per 1000 Living.																
Average in the 25 years 1875-99	31·1	32·3	32·2	23·8	31·3	30·7	28·7	38·0	42·9	28·9	37·2	37·7	34·2	30·1	23·7	35·6*	36·6
1875 ...	33·9	35·4	35·2	26·1	31·9	31·2	31·2	39·9	45·0	32·0	40·6	40·7	36·6	32·5	25·9	?	37·7
1876 ...	34·8	36·3	35·6	26·4	32·6	31·8	30·8	40·0	46·3	33·0	40·9	40·7	37·1	33·2	26·2	?	39·2
1877 ...	34·4	36·0	35·3	26·2	31·8	31·8	31·1	38·7	43·6	32·3	40·0	39·9	36·6	32·3	25·5	?	37·0
1878 ...	34·0	35·6	34·9	25·1	31·7	31·1	29·8	38·6	43·1	31·6	38·9	38·7	36·1	31·5	25.2	?	36·2
1879 ...	33·3	34·7	34.3	25·2	32·0	32·0	30·5	39·2	45·8	30·8	38·9	39·0	36·7	31·5	25·1	?	37·8
1880 ...	32·7	34·2	33·6	24·7	31·8	30·7	29·4	38·0	42·8	29·8	37·6	37·8	35·5	31·1	24·6	?	33·9
1881 ...	32·5	33·9	33·7	24·5	32·3	30·0	29·1	37·7	42·9	30·0	37·0	37·0	35·0	31·4	24·9	?	38·0
1882 ...	32·3	33·8	33·5	24·0	32·4	30·9	29·4	39·1	43·8	29·1	37·2	37·6	35·3	31·2	24·8	?	37·0
1883 ...	32·0	33·5	32·8	23·5	31·8	30·9	28·9	38·2	44·8	28·8	36·6	37·1	34·3	30.5	24·8	?	37·1
1884 ...	32·2	33·6	33·7	23·9	33·4	31·0	30·0	38·7	45·6	28·5	37·2	37·6	34·9	30·5	34·7	?	38·9
1885 ...	31·6	32.9	32·7	23·5	32·6	31·3	29·4	37·6	44·8	28·0	37·0	37·7	34·4	29·9	24·3	?	38·4
1886 ...	31·5	32·8	32·9	23·2	32·6	30·9	29·8	38·0	45·6	28·0	37·0	37·7	34·6	29·6	23·9	?	36·8
1887 ..	30·7	31·9	31·7	23·1	32·0	30·8	29·7	38·2	44·2	28·0	36·9	37·7	33·7	29·4	23·5	?	38·8
1888 ...	30·1	31·2	31·3	22·8	31·7	30·8	28·8	37·9	43·8	27·8	36·6	37·4	33·7	29·1	23·1	36·6	37·4
1889 ...	30.0	31·2	30·9	22·7	31·3	29·7	27·7	37·9	42·7	27·6	36·4	37·1	33·2	29.5	23·0	36·7	38·1
1890 ...	29·2	30·2	30·4	22·3	30·6	30·3	28·0	36·7	40·3	26·4	35·7	36·6	32·9	28·7	21·8	34·8	35·7
1891 ...	30·4	31·4	31·2	23·1	30·9	30·9	28·3	37·0	42·3	27·8	37·0	37·7	33·7	29·6	22·6	35·8	37·0
1892 ...	29·5	30·4	30·7	22·5	29·5	29·6	27·0	36·2	40·4	27·4	35·7	36·3	32·0	28·9	22·3	36·4	36·1
1893 ...	29·8	30·7	30·8	23·0	30·5	30·7	27·4	37·9	42·6	27·7	36·8	37·5	33·8	29·5	22·3	36·1	36·4
1894 ...	28·8	29·6	29·9	22·9	30·1	29·8	27·1	36·7	41·5	27·1	35·9	36·6	32·7	29·0	22·3	35·3	35·4
1895 ...	29·4	30·2	30·0	23·2	30·0	30·6	27·5	38·1	41·9	27·1	36·1	36·9	32·8	28·5	21·7	35·4	34·7
1896 ...	29·0	29·6	30·4	23·6	30·3	30·4	27·2	38·0	40·5	27·9	36·3	36·9	32·7	29·0	22·5	36·3	34·7
1897 ...	28·9	29·5	30·0	23·5	29·7	30·0	26·7	37·5	40·3	28·1	36·0	36·5	32·5	29·0	22·3	34·6	34·6
1898 ...	28·7	29·3	30·1	23·2	30·2	30·3	27·1	36·2	37·7	38·4	36·1	36·7	31·9	28·6	21·8	33·8	33·4
1899 ...	28·5	29·1	29·8	22·9	29·8	30·9	26·4	37·1	39·3	28·9	35·8	36·3	32·0	28·8	21·9	34·8	33·8
1900 ...	28·2	28·7	29·6	22·7	29·8	30·1	26·9	...	39·3	28·6	35·6	36·1	31·5	28·9	21·4	34·4	32·9
	Deaths per 1000 Livin .																
Average in the 25 years 1875-99	19·1	19·3	19·4	18·1	18·5	16·7	17·1	28·8	32·7	20·8	24·4	23·9	20·6	20·3	22·0	30·4*	26·7
1875 ...	22·1	22·7	23·3	18·5	21·0	18·8	20·3	30·0	37·0	24·2	27·6	26·6	25·6	22·7	23·0	?	30·7
1876 ...	20·4	20·9	20·9	17·3	19·7	18·0	19·6	29·8	35·9	24·3	26·3	25·4	23·5	21·9	22·6	?	28·8
1877 ...	19·9	20·3	20·6	17·5	18·7	16·9	18·7	31·6	36·6	23·6	26·4	25·6	22·2	21·1	21·6	?	28·3
1878 ...	21·1	21·6	21·2	18·6	18·5	15·8	18·1	31·6	37·4	23·5	26·2	25·8	23·0	21·5	22·5	?	29·1
1879 ...	20·5	20·7	20·0	19·6	19·8	15·0	16·9	29·9	35·7	22·7	25·6	24·7	22·5	21·9	22·5	?	29·8
1880 ...	20·4	20·5	20·5	19·8	20·4	15·9	18·1	29·8	37·2	22·0	26·0	25·5	23·5	22·3	22·9	?	30·8
1881 ...	18·7	18·9	19·3	17·5	18·3	16·8	17·7	30·6	34·4	22·6	25·5	24·9	21·5	20·9	22·0	?	27·6
1882 ...	19·3	19·6	19·4	17·3	19·3	18·4	17·4	30·8	35·3	22·2	25·7	25·4	20·7	20·2	22.2	?	27·5
1883 ...	19·6	19·6	20·2	19·2	18·4	17·1	17·3	30·1	32·2	20.6	25·9	25·6	21·8	20·8	22·2	?	27·5
1884 ...	19·4	19·7	19·6	17·5	18·4	16·4	17·5	29·4	31·0	20·4	26·0	25·7	22·2	20·9	22·6	?	26·8
1885 ...	19·1	19·2	19·3	18·4	17·9	16·5	17·8	30·1	31·8	21·4	25·7	25·4	21·0	20·1	22·0	?	26·9
1886 ...	19·2	19·5	18·9	17·8	18·2	16·1	16·6	29·4	31·7	20·8	26·2	26·1	21·8	21·1	22·5	?	28·6
1887 ...	19·0	19·1	19·0	18·2	18.3	16·0	16·1	28·9	33·8	20·3	24·2	23·8	19·7	19·3	22·0	?	27·9
1888 ...	18·1	18·1	18·0	17·9	19·7	16·9	16·0	29·2	32·0	20·0	23·7	22·8	20·4	20·1	21·9	30·3	27·4
1889 ...	18·1	18·2	18·4	17·4	18·6	17·4	16·0	27·3	29·9	20·0	23·7	23·2	20·1	19·1	20·7	30·9	25·5
1890 ...	19·4	19·5	19·7	18·2	19·0	17·9	17·1	29·4	32·4	20·8	24·4	24·0	20·5	20·6	22·8	32·7	26·2
1891 ...	20·0	20·2	20·7	18·4	20·0	17·5	16·8	28·2	33·1	20·4	23·4	22·9	20·7	21·0	22·9	32·0	26·0
1892 ...	19·0	19·0	18·5	19·4	19·4	17·8	17·9	28·8	35·1	18·8	24·1	23·4	21·0	21·8	22·8	31·3	26·1
1893 ...	19·0	19·1	19·3	17·9	18·8	16·3	16·8	27·2	31·2	19·9	24·6	24·2	19·2	20·3	22·5	30·3	25·1
1894 ...	16·8	16·5	17·1	18·2	17·4	16·9	16·4	27·8	30·5	19·9	22·3	21·8	18·5	18·6	21·2	31·1	24·9
1895 ...	18·7	18·7	19·4	18·4	16·8	15·7	15·2	27·7	29·7	19·1	22·1	21·8	18·6	19·5	22·2	29·4	25·0
1896 ...	17·0	17·0	16·6	16·6	15·6	15·2	15·6	26·4	28·9	17·7	20·8	20·7	17·2	17·5	20·0	29·9	24·0
1897 ...	17·6	17·4	18·4	18·4	16·5	15·3	15·4	25·6	28·5	17·6	21·3	20·9	16·9	17·2	19·5	28·8	21·9
1898 ...	17·6	17·5	18·0	18·1	15·5	15·3	15·1	24·9	28·0	18 2	20·5	20·0	17·0	17·6	20·9	28·6	22·9
1899 ...	18·2	18·2	18·1	17·6	17·3	16·9	17·7	25·4	27·2	17·6	21·5	21·4	17·1	18·8	21·1	29·3	21·8
1900 ...	18·4	18·2	18·5	19·6	16·9	15·8	16·8	—	26·9	19·3	22·1	21·8	17·8	19·3	21·9	29·4	23·7

* Average of 12 years—1888-99.

There is one great fact, however, brought out prominently by a study, however brief, of this table. High birth-rates as a rule accompany high death-rates, and low birth-rates low death-rates; and to a large extent these are cause and effect of one another. In Austria and Hungary the birth-rates, though falling, are still higher than in any other European country (37 and 39 per 1000), and their death-rates are also the highest (25·4 and 27·2 in 1899). The statistics of Spain form an exception, but apparently the figures are not so full or accurate as for other European countries. And the reason why birth-rates and death-rates are not only casually, but also causally connected, is this. There is unfortunately a very large mortality proportionately at the early ages of life. Of 1,000,000 male children born over the average of the ten years 1881-90, only 760,167 would be found surviving at the end of five years or under five years of age. For females the number surviving would be 791,973. Roughly speaking, about one-fourth of the children born die in the first five years of life. In the first year of life, out of 1,000,000 males born 161,036, or 161 per 1000, died over the average of the ten years 1881-90. For female children the corresponding number was found to be 131,126, or 131 per 1000. Now a lowered and a lowering birth-rate, such as we see to have been going on over a number of years past, means that a smaller proportion of the population are alive at the more fatal ages than when the birth-rate was higher.

And this being so, a lowered birth-rate implies a lowered death-rate, even if no sanitary improvements take place in the condition of the people. When, therefore, we compare, as we often have occasion to do, the hygienic and sanitary conditions of one set of people with another, or, say, the health conditions of one town with another, we shall be omitting a very important factor if we do not take account of differences in the birth-rate of the towns. For the curious thing is that there often exist such differences even in towns quite near to one another. The fall in the birth-rate has been, as we have seen, continuous in England and Wales for many years past. But this fall, though applying generally to the country and to the United Kingdom, is much more accentuated in some places and in some parts of the country, and notably in some of the boroughs, than others. This is more marked in the West Riding of Yorkshire, *e.g.*, than in London. In Bradford the birth-rate is the lowest, and that by a good deal, of all the large towns (with populations of 200,000 and over) in the United Kingdom; and its birth-rate has been persistently falling for a long time. Thus in 1881 it was 33 per 1000; in 1885 it was 29; in 1891 it was 28·6; in 1895 it was 26·6; in 1900 it was 24·6; and in 1901 it was 23·1. In Leeds, only nine miles away, the birth-rate in 1901 was 30 per 1000; in Sheffield, 33·1; in Hull, 33·3; and in Newcastle, 32·1. All these towns have populations of over 200,000, and Bradford's birth-rate is considerably lower than that of any

of them. Only in Halifax and Huddersfield, of Yorkshire towns, each with populations of about 100,000, is the birth-rate lower than in Bradford, being 22·5 and 22·7 in 1901 respectively. In London in the same year it was 29, while the average of thirty-two provincial English towns was 29·8. The low birth-rates of some of the West Riding of Yorkshire manufacturing towns of late years is a curious and interesting fact.

A low birth-rate implies a low death-rate, and a high birth-rate implies a high death-rate. It might be argued, and probably with truth, that more care would have prevented a large proportion of the deaths that occur among infants and young children. Even with a low birth-rate, in some places the infant mortality is high. In Bradford in the ten years ending in 1900 the death-rate of infants under one year of age averaged 171 out of every 1000 born, and, as we have seen, the birth-rate is very low. Whatever the causes of this may be, wheresoever a low birth-rate and a high infantile mortality rate coincide, that portion of the population has laid on it the burden of accounting for the coincidence, and of shewing that it is not due to negligence. It is quite compatible with the view taken by the writer of this essay that the most important cause of mortality in general is wrong feeding, to note what Dr. Arnold Evans, the Medical Officer of Health for Bradford, says regarding the high infantile mortality in that city. "The high infantile mortality," he says in his report

for 1901, "is not to any great extent the result of the employment of women in factory work, but I am sure it is largely contributed to by a wide-spread ignorance of the management and feeding of infants among the working classes. In the returns prepared by the Inspector, such expressions as 'fed by breast for two months, and afterwards on Quaker Oats and lime water,' or 'pobbies,' are not uncommon; some infants were fed on all kinds of malted food, and one infant, which died at the age of ten months, and commenced life on humanised milk, was afterwards fed on 'Marshall's Malted Food, Scott's Emulsion, and brandy and cream,' and towards its end on 'veal and tea.'" In order to meet this deplorable state of things he recommends the establishment of "municipal milk depôts, where humanised milk may be obtained in amounts suitable for infants of various ages, and as cheaply as it can be produced." Information as to the proper methods of feeding infants, and the quantities and times for administration of their food, would also be greatly for the benefit of the public health, since even suitable food may be administered too often or too seldom. As a rule, however, it must be admitted, when all has been done, that the rearing of young children, like the rearing of young animals of all sorts, is a task accompanied by many difficulties, and even when much and continuous care has been exercised, some amount of failure is apt to be experienced. No doubt, many sensible and careful and well advised mothers rear all their

children, but we are struck with the reflection that some or many others must be careless, ignorant or incapable, or through economic or other causes may find the task beyond them, when we realise that about a quarter of all the children born in this country die before they are five years of age. In England and Wales in 1900, 209,960 deaths out of a total of 587,830 occurred among children under five years of age. That was about 36 per cent. In many years two-fifths or 40 per cent. of the total mortality has been returned as occurring among children under five years of age. In 1893, for instance, 569,958 deaths occurred in England and Wales, and of these 216,833, or very nearly two-fifths or 40 per cent., occurred among children under five years of age. As in many of the towns the proportion is the same, we can see how important an influence such a fact must exert on the general mortality returns. But about 25 per cent. of all the children born die before they are five years of age. If, therefore, a smaller number of children are born into the world than formerly (and that is the meaning of a low birth-rate), obviously there will be a smaller number of the population living to afford so high a mortality, and, therefore, the general mortality rate will prove lower, not only because of general improvement in the public health, but also because a smaller proportion of the population are living at what may be termed the very fatal ages. An illustration will make this clear. Suppose a town of 200,000 inhabitants has

a birth-rate of 30 per 1000, whereas formerly it had a birth-rate of 35. Obviously 6000 children will be born where 7000 used to be; that is, 1000 fewer children will be living in that town under the new birth-rate than under the old one during a given year. Now suppose that one-fifth of the children born died under one year old (which rate, I regret to say, is not only reached but even occasionally exceeded in some of our large towns), then 1200 children would die, as against 1400, had the former birth-rate been maintained. This would reduce the deaths by 200 in a year, or 1 per 1000 per annum in a population of 200,000, a diminution quite sufficiently marked to seem to justify the statement that that town was at a higher level of health than others.

Another point should not be lost sight of. A large proportion of all the cases of fever that occur, a large proportion of the zymotic diseases as they are called, happen among children under five years of age — a large number of cases of measles, scarlatina, diphtheria, diarrhœa, &c. Now, as under a lower birth-rate a smaller number of persons specially susceptible to these ailments are living at a given time, it follows that fewer of these cases will occur, and therefore that a lowered birth-rate lowers not only the general mortality as we have seen, but that it lowers also the zymotic or fever mortality. But the lowering of the zymotic mortality is almost by every writer pointed to as marking the general sanitary advance of the

country. From the considerations advanced, however, it is apparent that this is not necessarily so, that a mere diminution of mortality is not in itself necessarily a mark of improvement in the public health, that a mere diminution even in the mortality from fever may be capable of other explanation, and that many corrections may have to be made before we are in a position to estimate justly the state of the public health as shewn in the mortality returns of the Registrar-General or in those of local Medical Officers of Health.

For some reason, not apparent on the surface, the mortality among infants under one year of age has not improved as compared with the mortality at other ages in recent years. Comparing the ten years 1881-90 with 1871-80, we find the mortality for male infants under one year was 161,036 per million, or 161 per 1000, in 1881-90, as compared with 158 per 1000 in 1871-80. For females the corresponding numbers were 131 in 1881-90, as against 129 in 1871-80. For 1900 the infant mortality in England and Wales for both sexes was 154 per 1000. Various suggestions occur to account for this somewhat remarkable fact. Under five years of age, on the other hand, the improvement has been considerable, whether we compare 1881-90 with 1871-80 or with the average of the years 1838-54. For females the mortality under five years of age was :—For 1881-90, 208 per 1000; for 1871-80, 224; and for 1838-54, 236. For males the rates were 240, 253 and 263 respectively,

shewing a progressive diminution of mortality since registration began in England and Wales.

We have seen that 209,960 out of 587,830, or about 36 per cent., of the deaths which occurred in England and Wales in 1900, were of children under five years of age. On the other hand, 142,496 deaths, or about 24 per cent., occurred among persons over 65 years of age. That leaves 235,374 deaths, or about 40 per cent., as the number and proportion of deaths which occurred between five and 65 years of age. I take this number as it gives the proportion of deaths which occurred at what may be considered the active or working period of life. The number of the active population is indeed not so large as this, for before 15-20 years we can scarcely reckon school children among the active working members of the community. But from five to fifteen years of age mortality is very low, and so I have for convenience taken the 40 per cent. of mortality which occurs between five and 65 years of age into consideration as a whole. Of what did these 235,374 persons die? Of course a preliminary question is why did they die at all? If children pass through the dangers which beset them under five years of age, why should they not reach old age? Why at least should they not overpass 65 years, if it is too much to hope that they should reach the three score years and ten usually considered as the term of human life? It is for medicine to say why. Of course some accidents must be expected to occur. We cannot hope that

the whole population over five years of age should reach old age or even 65 years of age, although to take 65 years instead of 70 is to make easier the problem of medicine by five years of life. Let us assume that for various reasons, delicacy, for example, accident, and, in one word, unsuitability of environment, 20 per cent. or one-fifth of these 235,274 persons should fail to reach the age even of 65 years. That would still leave 188,300 deaths as occurring in England and Wales in 1900 between five and 65 years of age, who might reasonably have been expected to reach it. As to accident itself as a cause of mortality we do not find that it is an increasing factor. Legislation has been active in this direction as well as in others in defence of public health; and in compulsory fencing of machinery, defence from dangerous places, compensation for injury sustained in the prosecution of daily occupations, and in other ways, has taken what steps it could in the way of preventing death from accident and violence. I do not know that much stress need be laid on the figures, and there is no need to do so; but so far as can be made out there is no increase of mortality under the heading of accident among the people of England. Thus the deaths returned as from accident in 1868 were in the proportion of 30,867 to 1,000,000 deaths from all causes, while in 1893, a quarter of a century afterwards, they were at the rate of 29,615 to 1,000,000 deaths from all causes. In 1900 the proportion was 30,567. There is no increase of

mortality from this cause. From 1866-70 the deaths from accident or negligence were at the rate of 677·8 per million living; in 1876-80 the rate was 630·4; in 1886-90 it was 543·8; and in 1896-1900 it was 557·8. Still, if death from accident and violence is not increasing, but if anything diminishing in the country (and after all accident is a comparatively small cause of mortality), it will be interesting to inquire what are the principal causes of mortality in the population, especially at the middle or active portion of life. This I propose to do in the next chapter. Meantime, if we attempt to sum up the results of improved medicine and surgery since the year 1838, when the Registrar-General began to compile the figures for England and Wales, they appear to be the following. The fevers or zymotic diseases have fallen to about one half, and consumption by more than one half in their incidence and mortality during that time. These two great and very satisfactory results are aided, however, by a considerable fall in the birth-rate, and the three factors account for a diminution in the general death-rate of about 20 per cent. since 1838. The expectation of life has risen in the case of males from 39·91 years to 43·66 years, and in the case of females from 41·85 years to no less than 47·18 years, and an immense number of years of life amounting to many millions have been added to the population in each generation. The addition, however, has been made rather at the two extremes of life, children under 20, and old persons over 65,

having benefitted much more than those living between 25 and 65 years of age. The expectation of life has been raised in the case of females at all ages except from about 65-75, and for males except from 45 to 75 years of age; rather important exceptions, however. Keeping these changes and improvements in our minds, we shall be better able to estimate their causes, after examining the causes of mortality from five to 65 years of age, as is now to be done.

CHAPTER II.

The Principal Causes of Mortality in England and Wales.

IN 1900, in England and Wales, the most frequent cause of death, taking the whole population at all ages, was bronchitis. It accounted for 54,580 deaths out of the 587,830 occurring in that year. This is no unusual proportion, as, year after year, bronchitis heads the list among the causes of mortality. In 1899 bronchitis accounted for 51,206 deaths; in 1898, for 46,261; and so on. If we calculate the proportion of deaths occurring from this cause to a million deaths from all causes, we shall find that in 1900 bronchitis accounted for mortality in the proportion of 92,850 deaths out of each million that occurred. This is usually called the proportional number, and it may be well to make use of this expression. The next commonest cause of death is pneumonia or inflammation of the lungs; single pneumonia when only one lung is inflamed, double when both are affected. The number of deaths that occurred from pneumonia in England and Wales in 1900 was 44,300, and the proportional number was 75,362. Between them, these two diseases accounted for at the rate of

168,212 deaths out of every million of deaths that occurred. These diseases are generally said to be due to cold. No doubt exposure to cold is very often the immediate exciting cause of them. But there is very strong reason to believe—and I hope to be able to adduce probable if not even demonstrative evidence to this effect — that not only these diseases, but many others also, have their predisposing causes in errors of diet. "How few diseases," says Professor Stewart in his Manual of Physiology, "are not in some degree alimentary." As to the opinion, however, that bronchitis and pneumonia are due to exposure of the body to cold, it must be said that these affections are not by any means experienced only in cold weather. Patients often suffer from severe attacks of them also in summer. The true statement regarding the effects of heat and cold in inducing these diseases would be, I suppose, that, in those predisposed to them, cold is often the exciting cause, and so is heat, but that without the predisposition neither cold nor heat, at least in the moderate amounts of them experienced in ordinary life in England, would induce them. Humanity is intended and fitted to endure without damage moderate changes both in heat and cold. And predisposition to disease—what is that? Predisposition is inverse resistance; it is weakness; and it can, I think, in the large majority of cases, be defined in terms of the influence on the economy of air, food, and exercises or movements. What is meant by that statement

is this. If the economy is treated by a proper amount of food, neither too little nor too much; if the air supplied to it is moderately pure and abundant; and if exercises or movements of the body are properly followed, neither over-exercise nor under-exercise being indulged in, then the predisposition of the body to disease will be found to be greatly lessened, or otherwise the resistance to disease will be greatly increased. If, on the other hand, the body has either too much food or too little; if the air breathed is impure and in excess or deficiency; and if the muscular movements used are too few or too many, then the predisposition of the body to disease will be found to be greatly increased, or otherwise its resistance will be greatly diminished. Predisposition is inverse resistance; the greater the predisposition the less the resistance, and the less the predisposition the greater the resistance to outside influences. Heat and cold are of course relative terms. If heat is assumed to be the positive condition, then cold is only a less amount of heat, so that exposure to heat and cold only means, when we think about it, exposure to more heat or to less heat. It might be thought that "cold" might be defined as a degree of heat less than the normal temperature of the body; but if this were so, then we should be obliged to say that, in the English climate at all events, the weather is almost always cold, because the temperature of the air is very rarely indeed so high as 98·4°F. In hydropathic establishments, and in some

of the literature appertaining thereto, the term "cold" is applied to water of a temperature of 60°F. or less. We are quite unable to alter our climate; we may have a change of climate, indeed, that is a few of us can, very few comparatively; but we are sometimes well, and, on the other hand, sometimes ill, in all kinds of weather, so that it is not the weather only which affects us, a more important factor being the state we are in, or the predisposition of the body to disease. But to say that a man was specially predisposed to any disease, to bronchitis, for example, or pneumonia, is equivalent to saying that his resistance was so low that an amount of exposure less than would have been necessary in the case of another man sufficed to induce in him an attack of bronchitis or of pneumonia. But in reference to this, the important point to get into our minds, and to keep there, is that predisposition can be altered by changes effected between the organism and its environment, and particularly by alterations in the food habits. No doubt alterations of the environment in other particulars also have their influence. It is well-known that through accustoming oneself to cool baths, to open windows, and to moderate and well regulated exercises, the resistance of the body to disease can be greatly increased, or, in other words, that its predisposition to disease can be greatly lessened. But while this is so, no doubt, the chief means by which predisposition to disease can be lessened seems to consist in a proper

arrangement of the dietary, so that neither too much nor too little is taken. How much that is, and how often it ought to be administered, attempts will be made to state later. Meantime, it should be stated that, as has been said long ago, the quantity and kinds of food, and the proper times for taking it, cannot be stated to a nicety (*εἰς ἀκριβείην*, to use the expression of Hippocrates), and the quantity proper to be taken depends on several qualifying circumstances.

After bronchitis and pneumonia, the cause of mortality shewing the next highest proportional number is phthisis or consumption of the lungs, whose proportional number is 73,128 for the year 1900. It may be contended that to this ought to be added the proportional numbers of other forms of tubercular disease, so that we may realise the full importance of this cause of mortality. If this were done, and if the proportional number of tabes mesenterica, or consumption of the bowels (10,166), and of tubercular meningitis, or consumption of the brain, as it is often popularly called (10,879), and also the proportional number of what the Registrar-General calls "other forms of tuberculosis" (10,112), were added, the proportional number of tuberculosis in general would stand at the very high figure of 104,285, or over one-tenth of the total mortality. Of course, if this were done, it would be necessary to take the proportional number, say, of the whole of the respiratory diseases in order to have a fair comparison

of the mortality due respectively to tubercular and to simple respiratory diseases. In this case, tuberculosis, accounting for 104,285 deaths out of a million from all causes, would be compared with 182,355 caused by respiratory diseases. Diseases of the circulation again account for 93,857 deaths out of every million that happen. To this, however, I think the proportional number of apoplexy ought to be added, because, although apoplexy shews itself chiefly in nervous phenomena such as paralysis and coma, still it is essentially dependent on degeneration and rupture of vessels, changes in whose condition really belong to diseases of the circulatory apparatus. This number, 32,509, if added to the proportional number of diseases of the circulation, would raise the latter to 126,366, a very high figure indeed.

Of "old age" as a cause of mortality, the proportional number (next highest on the list) is 54,056 for the year 1900. In a community quite healthy, or even approximating to that condition, this cause of mortality ought to be very much higher. It is, in fact, a task for medicine to explain why only about 5½ per cent. of the total mortality is due to old age. We ought to have a very much higher proportion. As to the influences of food and feeding on the attainment of old age, a considerable amount of evidence ought to be available. If, at least, we can connect the causation of some of the diseases already named, and which often prevent the onset of "old age" — if we can connect those

diseases or any of them with improper feeding, we shall incidentally shew the connection between proper feeding and the attainment of old age. Respiratory diseases, it appears to me, diseases of the circulation, and tubercular diseases, all seem directly connected with mal-nutrition; and I hope to adduce evidence in support of this view in the sequel.

Next after old age, as a cause of mortality, comes cancer, whose proportional number is 45,457 for 1900 in England and Wales. I dwell at some length on cancer in a later part of this essay, and therefore need not do more here than say what I have no doubt at all is the simple truth, viz., that from first to last, from its commencement in indigestion, loss of teeth, anæmia (triphthæmia, or catatribæmia rather), pallour, fatigue, rheumatism, and constipation, to its later manifestations in epithelial and connective tissue overgrowth with its limited molecular caries, or more general necrosis, or molecular or general death of larger parts of the body and their elimination or exfoliation with offensive putrefying odour into the outer world, and finally to its culmination in hæmorrhage or the effusion of immense quantities of usually bloody serum into the body with or without the development of micro-organisms; from first to last cancer is a disease of mal-assimilation of food, and that mal-assimilation is caused not by too little but by too much food, and particularly by too frequent feeding. When this process has gone on for many years, it is, as a rule,

impossible that cure can be effected. We might as well ask that the effects of a devastation should be undone, that the drowned in a flood should be brought to life again, or the victims of La Souffrière or Mont Pelèe should be requickened. Prevention might have been easy and simple had steps been taken at the proper time, but when that time has been allowed to go by, cure is impossible. The medical profession cannot, nor can any body of men, clerical or lay, accept the role which the public are too apt by implication or avowal to present to them, viz., to relieve humanity from the effects of their action without altering their habits. Not in this way is power over nature attained; and if we refuse to put some necessary restraint on ourselves when we have still the power and opportunity to do so, we shall certainly find it vain to stretch out hands too late in mute appeal to surgery or medicine, demanding that they should cure the incurable or re-vivify the dead. This explanation may not perhaps account for all the cases of this sort that are met with in the practice of medicine; a certain small proportion will still perhaps be inexplicable, are there always must be anomalies and exceptions to every rule; but if nine-tenths of the cases, or nineteen-twentieths of them, are so accounted for, and if an alteration in our habits should enable us to prevent so large a proportion of incurable diseases, we might reasonably say that we had solved the dreadful problem of the prevention of cancer. That this is within our reach I have no doubt. Cure may be practically

impossible (for what is the good of talking of cure when the most we can do is to save say one, two or three per cent. of the cases?), but prevention is easy and simple.

Diarrhœa and dysentery, whose proportional number for 1900 in England and Wales was 38,984, are next on the list of the causes of mortality. These are diseases markedly dependent on improper feeding. I should take it that this is a case in which there will be little or no difference of opinion among experts, not, of course, to the view that improper feeding is the *only* cause of the incidence of and the mortality from those affections, but certainly that it is much the most important one. Diarrhœa is a large cause of mortality among infants. In 1900, of the 22,916 deaths attributed in England and Wales to diarrhœa and dysentery, 20,542, or about ten-elevenths, or, say 90 per cent., occurred among children under five years of age; and it is the opinion of medical officers of health that a very large proportion of infant mortality is due to improper feeding. I think I need hardly quote authorities to prove a statement regarding the truth of a view so generally held as this.

Convulsions, whose proportional number for 1900 is 31,475, comes next on the list. This affection occurs almost entirely among infants. Of 18,502 deaths attributed to this cause in the year mentioned, 18,344, practically all of them, occurred among children under five years of age. By common consent these diseases are often attributed

to errors of diet. Convulsions often appear in the course of an attack of diarrhœa or of some other acute illness, and many of them can be connected with errors of diet.

We next come on influenza with a proportional number of 27,636 deaths to a million from all causes, measles with 21,622, whooping cough 19,507, and diphtheria 15,897. These all belong to the group of zymotic diseases, whose proportional number (exclusive of cholera, diarrhœa, &c.) amounted to 101,600 in 1900, one of the very largest groups of causes of mortality. Further on in this essay I adduce (Chap. IV.) what seems reasonable evidence to shew that these diseases are probably also somewhat dependent on wrong feeding, although it is customary to attribute them almost wholly to vitiated air. I do not deny, however, that the latter is also an important cause of the fevers. I also shew that for ten years past the fevers or zymotic diseases have not diminished in England and Wales, although we certainly have not intermitted our efforts towards the obtaining of better sanitation in the restricted sense of that term. For the three years 1890, 1891 and 1892, the proportional numbers of the zymotic diseases in England and Wales were 130,200, 133,960, and 146,579 respectively. In 1897 the proportional number was 148,908. In 1898 it was 158,343, and in 1899 it was 154,450. The proportional numbers are higher at the end of the ten years than they were in the beginning of the period. It appears either that we are in course of exhausting

the benefit which we may expect to receive from sanitation in the restricted form in which alone it is customary, unfortunately, to use the term, or that we have overlooked a main or important part of the cause of the zymotic diseases. I believe myself that weight must be attached to both of these suggestions. It is certainly strange that if fevers are dependent on bad air and overcrowding, &c., they should not only not continue to fall in frequency in response to our continuous efforts to improve conditions in these respects, but that their proportional number should actually increase. I have little doubt that an important factor in their causation has been overlooked, and I have equally little doubt that that factor is the food habits of the people, the too frequent and too abundant meals which they habitually ingest both into themselves and their children. When Sydenham suggested, at the time of the English revolution, that fevers came *e visceribus terrarum*, out of the bowels of the earth, it might have been suggested to him that perhaps a main part of the cause arose *e visceribus hominum*, out of the over-loaded digestion of men. The idea is not so unlikely as might have been thought. It is not even new. The sanitary genius of Moses was not bound down by the notion that plague depended upon bad air, for, as we have seen, he attributed the attack that his people sustained from that zymotic disease, to the quails which they stood up and gathered during a whole day and a night and all the next day, and which no doubt

they ate in great excess. But after burying his dead at Kibroth-hattaavah, and apparently believing that the ground and the air were infected with the pestilence, he moved his camp also to Hazeroth, so taking steps to get rid of the double cause of improper feeding and infection of land and air. If modern sanitarians would follow this sagacious and broad-minded example, they would no doubt find that instead of the zymotic diseases gaining on them in spite of their efforts, they would have the satisfaction of seeing that their sanitary efforts were prevailing against the zymotic diseases. And, I think, if they were dealing with the whole of their causation, and not with a part of it only, they would find that they could so advise the people as that they might get rid not of the half only of the number and incidence of these diseases, but of a proportion somewhere between three-quarters and nine-tenths. Under specious phrases like the progress of science and the like, we seem far too ready complacently to assume that we know everything while the people who went before us knew nothing; but the assumption is not always true in either direction.

To diseases of the digestive system, more particularly diseases of the mouth, stomach, intestines, liver, spleen, lymphatics, &c., the proportional number of some 61,801 deaths were attributed in 1900 in England and Wales; and if to these the proportional number of deaths due to diseases of the kidneys and to Bright's disease—some 21,200—be added, we reach the large proportional number of

83,000 as that of the diseases of the digestive system. If we take a wider view of things, no doubt—and this is in one word the thesis of the present essay—diseases directly or indirectly attributable to the digestive system are very much more numerous than this; but this is the proportional number given by the Registrar general as applying to England and Wales for the year 1900; and it forms about a twelfth of the total mortality.

The only remaining important group of diseases to which mortality is attributable is diseases of the nervous system. It is a little difficult to gauge its value or to state its proportional number for various reasons. Its proportional number includes apoplexy, for instance, in the Registrar-General's return, but I have already said that I think apoplexy (accounting for a proportional number of 32,509 deaths in 1900) would be better classified under diseases of the vessels, since it is rupture of the arteries which is the main cause of apoplexy; and the brain softening and degeneration following are consequences of this and of the softening changes which occur in the blood-clot effused on to the surface or into the substance of the brain or other parts of the nervous system. The nervous system, in fact, being the organ of the controlling power in the body, has a very high measure of vitality, as if it were the intention of nature to preserve it to the last extremity. In death from direct starvation, for example, while other parts of

the economy lose great parts of their weight, the nervous system scarcely wastes at all. The significance of this, though not the existence of it, has been in a manner discovered by Dr. Dewey, of Meadville, Pennsylvania, who quotes from Dr. Burney Yeo statements, which he has entitled "Nature's Bill of Fare for the Sick." Besides quoting it in the text of his book, "The True Science of Living," Dr. Dewey places it in capital letters in the frontispiece of his book. He shews that in illness, when we are using up the materials accumulated in our bodies (and the same is true in death due to direct starvation, as when sailors, *e.g.*, are confined in a boat without food) we may use as much as 91 per cent. of our fat (practically all of it), that of muscle we may use as much as 30 per cent., that the spleen, a blood-making gland, may waste to the extent of 63 per cent., the liver as much as 56 per cent., and the blood itself be absorbed to the extent of 17 per cent. of its total amount. That in these circumstances the brain and nerve centres scarcely waste at all is surely a most significant fact, and is closely related to another of scarcely less importance, viz., that the nervous system is not particularly liable to suffer from direct disease of its proper substance. It is its vessels which become diseased, and, by their rupture, indirectly destroy its controlling power; or it is its fibrous coverings which become infiltrated with gouty or rheumatic exadation, and so lead to defect of its functions, to excess of its

functions, or to such irregularity of its functional power as compels us often to send persons to asylums where they can receive more care than it is possible for them to obtain at home. Considerations of the same kind incline me to think that even "hemiplegia and brain paralysis," to which the Registrar-General attributes a proportional number of mortality in 1900 of 10,384, ought to be also relegated to the diseases of the vessels of the brain, that is, to diseases of the circulation. However, I have still retained it among diseases of the nervous system, and I find that in this case, but deducting the proportional number of apoplexy (32,509), of convulsions, which is mostly a disease of infancy due to improper feeding (31,475), and of laryngismus stridulus, also a disease of infancy and early childhood, and mostly, I think, catarrhal, and due also to wrong feeding (742), there remains 47,917 as the proportional number of the mortality attributed by the Registrar-General to diseases of the nervous system for England and Wales for the year 1900. The causes enumerated, then, bronchitis, pneumonia and respiratory diseases in general, consumption of the lungs and other forms of tuberculosis, diseases of the circulation, cancer, convulsions, the zymotic diseases, diseases of the digestive system, including diseases of the kidneys and Bright's disease, and diseases of the brain and nervous system, account for the bulk of the causes of mortality among the population of England at all ages. If it can be

shewn, or if good reason can be advanced for the opinion, that these causes of mortality and disease are more aggravated by improper food habits than by any other means, and if, therefore, more improvement can be effected in the incidence of these diseases on humanity by alteration of the food habits of the people than in any other way, or even if it can be shewn to be probable that such alterations in food habits would be likely to diminish the virulence and power of these causes of disease and mortality, the task which I have set before myself in this essay will have been accomplished. For then it will have been incidentally shewn that a great part of the causes of the evils from which we suffer is diminishable by our own exertions, and can be reduced by better management of ourselves.

We have not yet, however, received the answer to our question—what are the chief causes of mortality among the active portion of the population living between the ages of 5 and 65 years? I am sorry to have to beg the reader's attention still to what he may consider details of dry arithmetic, but if I do so for yet a little while, it is because I am afraid I cannot put the problem before him in any easier or simpler way. Happily the study already given to the causes of mortality in general among the English population has rendered examination of the causes of mortality between ages 5 and 65 years by so much easier, and our next chapter, therefore, will be a short one.

CHAPTER III.

The Principal Causes of Mortality in England and Wales between the Ages of Five and Sixty-five Years.

IN the year 1900 there were in England and Wales, as has already been said, 587,830 deaths, and of these 209,960, or over 35 per cent. occurred among children under five years of age. This is a large proportion of the mortality, but it is not at all an unusual one. If anything, indeed, it is lower than usual. In 1893, for instance, there occurred in England and Wales 569,958 deaths, and of these 216,833, or about 40 per cent., took place among children under five years of age. Year after year it is the same, from one third to two fifths of all the deaths that occur in the country happening under five years of age. In our towns it is the same. In 1901 4,701 deaths were registered in Bradford, and of these 1,512, or 32 per cent., occurred among children under five years of age. There being thus about 32 to 40 per cent. of the total mortality under five years of age, that proportion of the mortality may be left out of account. As to the causes of that mortality,

however, my general argument is not weakened but strengthened, it being the opinion of medical officers of health that a great part of its cause is improper feeding. If this is so under five years of age, and as no noticeable break or gap occurs between the modes of management of children under five years of age and those over that age, or at subsequent ages, it is probable that mortality is induced by the action of similar causes at all ages, and therefore that the view which attributes a large proportion of the mortality at more advanced ages to wrong feeding is *prima facie* at least not improbable. Let us now look at the mortality at the other extreme of life. I have taken 65 years and not 70 as the point from which to examine the mortality because it is an easier problem for medicine to prolong life to 65 than 70, although I do not for a moment suggest that we ought to let 65 years of age form the limit of our ambition. If medicine succeeded in raising the age at death to 65 years, her ambitions would very soon rise to 70 and beyond. Meantime it seems hopeless to talk about such a thing, so very little has yet been done towards increasing life at the active and useful ages. In 1900, then, there died in England and Wales 142,496 persons over 65 years of age out of the 587,830 deaths occurring that year. That was about 24 per cent., and this forms year after year about the proportion of the population who reach not 70 years of age, but 65. In the towns again it is the same. In 1901, out of

the 4,701 deaths registered in Bradford, 1,038, or 22 per cent., attained the age of 65 years or upwards. On the assumption that life is worth living, this does not seem to me a satisfactory result. Further, if life could be prolonged in health and activity and interest, the objection frequently felt by persons who live long, to the solitude experienced by them in the loss of their friends would not count for so much, since the same causes which might lead to the prolongation of their life would lead to the longevity of their friends. Deducting, then, the 209,960 deaths under five years along with the 142,496 which occurred over 65, there remain 235,374 deaths to be accounted for between five and 65 years of age—which we may term the deaths at the active stages of life. From what causes did these deaths occur? In the order of their frequency, and therefore of their importance, we gather from the Registrar-General that they are the following:—

1. The most numerous group is that attributed by the Registrar-General to constitutional diseases which includes consumption of the lungs and other forms of tuberculosis, cancer, and rheumatism. This group accounts for 71,004 deaths out of the 235,374 to be accounted for. Practically it is tuberculosis and cancer which are to be considered, for the former accounted for 48,931 deaths between five and 65 years of age, and the latter for 17,026. Rheumatism and gout were not *directly* large causes of mortality,

although it would be very unwise to infer from this that these are unimportant diseases. Both rheumatism and gout are very important and very dangerous conditions of body indeed, but they are so far more in relation to what they mean and to the evils which they portend as about to ensue in the body, than in themselves. This quality is shewed by other diseases besides rheumatism and gout, and is, indeed, a characteristic of many of the minor ailments, especially, for instance, indigestion, in which nearly all, or at least a very large proportion of diseases have their origin.

2. The next most important cause of death between five and 65 years of age is diseases of the circulation, I ought to say that I have added the deaths caused by apoplexy to those attributed by the Registrar-General to diseases of the circulation for reasons mentioned in the previous chapter, and have therefore removed the deaths due to apoplexy from those attributed to diseases of the nervous system. Diseases of the circulation, then, and apoplexy account for 40,332 deaths out of the 235,374 in question. Apoplexy alone accounted for 19,110 of these deaths, so important a cause of mortality is it.

3. The next largest cause of mortality at these active ages of life is diseases of the respiration, which account for 36,321 deaths. Of these bronchitis and pneumonia together accounted for 31,383 deaths, so that the deaths due to other diseases of the respiratory system are comparatively

unimportant. Diseases of the respiration are, among persons living at the active ages of life, as well as among the population in general at all ages, a very important cause of mortality.

4. The next most important cause of death, according to the Registrar-General, is "old age," which accounts for 31,146 deaths under 65 years of age. I have already said that all would like to see this cause of death greatly increased, and to see life lived as it ought to be, till it determined in decay of nature rather than in succumbing to disease. "Old age," in fact, under 65 years of age, and particularly under 60 or less, would appear to be a misnomer. If persons succumb to "old age" at such ages, it is likely, on the face of it, that they have in some way mismanaged themselves, so as to make themselves old before their time, and it will be well before making any other inquiry to try to see whether a main cause of this is not over-filling of the body with food and drink, so clogging its actions and bringing its powers to a stand-still.

5. The cause next in importance of mortality between five and 65 years of age in England in 1900 was the zymotic diseases. We saw how very important a cause of death this set of diseases is among the whole population at all ages; and now we find that between five and 65 years of age it accounts for 29,766 deaths out of the 235,374 to be accounted for. This would give a proportional number of 126,000 out of a million deaths

from all causes, or about an eighth of the total mortality.

6. The next most important cause of death between five and 65 years of age is the diseases of the digestive system, which in 1900 accounted for 14,638 deaths out of the 235,374 which occurred in England and Wales in that year. This is still a large proportion, although if the arguments on which I have relied are sound, this number, large as it is, by no means represents the number of deaths due to disorders of the digestion. As it is, however, it represents a proportional number of 62,000 out of a million deaths from all causes.

7. Lastly, deducting apoplexy (a very important cause of death, as has been shewn), diseases of the nervous system accounted for 8503 deaths out of the 235,374 between five and 65 years in England in 1900.

From these seven causes of death, we find that 231,710 deaths are accounted for, out of the 235,374 in question in England and Wales in 1900, so that the remainder, due to parasitic diseases, dietetic diseases, &c., is very trifling. The class of dietetic diseases, I may say, however, includes the deaths from chronic alcoholism and delirium tremens, and is put at the too low figure of 3638 at all ages for England in 1900. As chronic alcoholism causes disease in various organs, as the liver, kidneys, lungs, heart, brain, and, in fact, in almost all organs, deaths that ought to be set down to alcoholism are far more frequently referred to diseases of those organs, so

that the Registrar-General's returns are no guide, and do not even offer an approximation to the number or proportion of deaths due to this cause. It is quite natural, of course, that relatives should not like to have the deaths of their friends put down to alcoholism, but their unwillingness to let this be done must not be allowed to blind us to the real facts of the case. Chronic alcoholism no doubt accounts for a frightful amount of mortality in England if the truth could be known; and no doubt also it is a most shocking cause of death, since no other cause equally lowers the power of the intellect in the government of men and women, sears their conscience and blunts their sense of refinement, or shuts their heart to the claims of those dependent on them. Bad as alcoholism is in these respects, however, and terrible in so many ways, it is not by a long way so potent a cause of mortality or of disease as improper feeding; and I cannot help thinking that if the teetotallers, instead of confining their attention to the evil effects of alcoholic drink alone, had widened the scope of their inquiries and extended them to the effects of intemperance in the use of food also, they would have effected much more good than they have yet succeeded in doing. There are passages in the writings of well-known medical writers on the evil effects of alcohol, referring a long succession of bodily evils to the effects of alcoholism, every one of which I have seen, and that not once, but over and over again, occurring in the life history of

teetotallers who were perfectly sincere in their statement that they abstained entirely from alcoholic drink. I am very sorry to say this, because I recognise to the full the good intentions of teetotallers and their sincere desire to benefit their kind in the best and highest ways, by inculcating on them the need for self control, and by shewing them how to achieve it. And I have endorsed the practice of the teetotallers by being one myself for years, so that I know of what I speak. No doubt they have shewn that alcohol is not necessary to persons in health, and that long and active and useful life is attainable without its use, while I think they have shewn also that childhood and youth are better without it. But nothing is gained by exaggeration, or by attributing to one cause what is often due to another, as, for instance, in the following passage, in which a distinguished physician said that "it is the prevalence of beer and spirit drinking, and consequent liver-clogging, which accounts for the widespread use and countless forms of patent pills such as Cockle's, Morison's, Holloway's, and others. These are taken by millions every week, and people find that if they do not take them they become bilious and unwell." And then he goes on to suggest that the chief reason for all this constipation and clogging, so very deleterious to the system, I freely admit, is caused by "gin-drinker's liver." But while this is sometimes a cause of the constipation which drives people foolishly to take aperients, by which,

nevertheless, they cannot cure it, surely it is not the only cause or the chief cause. It is impossible that constipation, so prevalent a condition in young and old and in middle-aged, and in all sorts and conditions of men and women, can be due to alcohol as its commonest cause. It is not compatible with common experience to say so. Thousands of the young women, for example, suffering in this way never take alcohol at all; and what it is due to far more than to anything else is excess not in alcohol but in food, and particularly to the widespread habit of taking too many meals. The *modus operandi* also of this habit and its consequences are quite plain. Too much chyle gets into the blood through over-feeding. The excess of nutritive material so introduced must be used somehow, and by means of it the muscular fibres of the bowel, and particularly the transverse or circular fibres, become overgrown or hypertrophied. The effect of this is to make these overgrown fibres contract too tightly, especially as the stimulus of too much nutritive material in the blood is still kept up by over-feeding. In this way the firm contraction of these fibres narrows the lumen or channel of the bowel, and prevents its contents from passing; or otherwise, we may say, causes constipation, and when some sort of action does occur, it causes the evacuation of those small knotty motions so common in this condition. Let the constipated young person, who is constantly taking sugar and spice and chocolates between her meals, give up

this pernicious habit; let her or him who is in the habit of taking four daily meals, try the effect of three for three months, and the probability is that he will be somewhat less constipated. If this does not cure him, however, let him take two daily meals for six months, and he will probably become regular, especially if he will take frumitty and cooked fruit at one of his meals, and the well boiled onion or fresh lettuce, tomatoes, &c., at the other. But if this is insufficient to put him right (but it will fail very seldom indeed) he will almost certainly become so on one daily meal. Alcohol is sometimes, no doubt, a cause of constipation, but sometimes, on the other hand, a little beer has the opposite effect —not that I recommend its use for that purpose, because I think the purpose can be better effected in the way suggested—but far the commonest cause of constipation is not alcohol but too much food.

The same statements are true, the necessary name-changes being made, as regards skin eruptions, which another medical authority attributes to alcohol. I do not deny that alcohol may cause acne and eczæma, for instance, that is, that beer may cause and that it often does cause spots and redness of the face and nose; but the large majority of the cases I have seen, occurring as they do in young women, who are greatly distressed by them, or in middle-aged women, many of whom do not touch beer or stout at all, much less brandy or other spirits, have been caused not by alcohol but by too

much starch and sugar in the diet, and these skin affections are really very easily treated by diminishing the number of meals taken and by restricting the use of these articles of food. I have seen many teetotallers die early, say between 50 and 60 years of age, not from their teetotalism at all, certainly not, but from wrong dieting of themselves, and particularly from taking too many meals. In fact, my experience leads me to say that teetotallers, as a rule, err in this way more than moderate drinkers, and having been deeply impressed by the truly shocking and degrading and demoralising effects of alcoholic drinking (let no one say I minimise these in the least, for I do not) have exaggerated its importance and extent, and so have attributed to alcohol many effects which are not due to it at all but to wrong feeding. Will any doctor seriously contend that say one glass of sherry or Madeira, taken at lunch, and one small whiskey taken at bed-time, in those cases where it suits—will any doctor seriously contend that such an amount of alcohol taken daily will shorten the life of the average man by a day? I do not believe it will, although at the same time I fully admit that it is not necessary to life or health, and although for my own part I quite expect to live to term without it. The moral and social grounds for abstinence from alcohol may lead persons justly to become teetotal themselves, and to inculcate total abstinence on other people—this style of argumentation attracts me very much, I must say—but we must be careful

not to transcend the bounds of truth and evidence even for a good cause, and we must also sanely attempt to attribute the effects of excess in all directions to their true causes, and to consider judicially the effects of too much food as well as of too much alcoholic drink.

Many strange things happen in life in the observation of those who notice what they see. I knew, for example, two brothers, one of whom lived to 75 years of age, the other attaining the age of 84. The former was a sober, righteous, and godly man, the centre of a numerous family of nephews and nieces, who looked to him as to a pattern for the help which young people so often need, and which he was able and willing to give. He died of a dilated, flabby, weak heart, brought on, I believe, partly by anxiety, but chiefly because he did not understand digestion, and because he ingested into his blood, habitually, more food than he required. The brother was such a rascal, and had so little command over himself that for 40 years he never went to bed sober, if he could go drunk. He was a pleasant, smiling, agreeable man to strangers, and when I saw him, at 83 years of age, he was as straight as a dart, and as nimble as a lamplighter. Yet, when he died the next year, so awful had been his family's experience of his vagaries, and so great the shame he had put them to, that his own daughter went down on her knees to thank God when she heard of his death. This was an exceptional case, no doubt. I may mention that

the longer lived brother, although he drank so hard, was a very spare eater. But how wrong it would be of me if I were to advise people, because this man lived in spite of his habits, that they should go and do likewise. And yet, would it be any more untrue to nature, or any less disproportionate to reality, than was, for example, the language of a compeer of my own, a medical man, who practised his profession for over 30 years, and who, all through his professional life, spoke of alcohol as "the accursed thing," advising all to abstain totally from it under peril of eternal loss? Or, to take another example. Many medical men believe that much damage is done, and many fatal illnesses are brought on by an excess of starch and sugar in the diet. But do we value their judgment if they advise total abstinence from these articles of food? Or at least is it not more rational of them if they advise, not complete abstinence, but such moderation as conduces to life, and health, and usefulness? The effects of the use and abuse of alcohol and of foods (and, I may add, of other things), may differ greatly in detail, but our attitude in respect of them must, it seems to me, be in principle always to remember that moderation is the only rule which never fails us.

CHAPTER IV.

The Causes of Disease or the Causes of the Causes of Death. Preliminary.

MORTALITY, being attributed to the diseases already named, or death being caused by them, an inquiry into the causes of these diseases may be otherwise considered as an inquiry into the causes of the causes of death. Death is, of course, the natural termination of life, but death ought to occur by a slow wearing out of the natural powers of the body. It very rarely does so; it occurs not from old age but from disease, for the most part. Now the causes of disease are usually considered under three divisions, termed respectively the predisposing, the exciting, and the proximate causes. The proximate causes of disease are two, and two only, viz.: shrinking and swelling, or contraction and dilatation. Organised bodies, like unorganised, have the power or quality of contracting or shrinking, and also of dilating, or swelling, or expanding, under the action of certain stimuli, but there is probably this difference between them, that unorganised bodies may actually shrink into smaller compass, so as to occupy less space at some times than others, under

the contracting action; while organised bodies, if they contract or shrink in one direction, usually expand or swell in another, so that it is doubtful if they occupy less space at one moment than at another. (Of course organised bodies grow and develope, and again shrink and become attenuated and old, so that at different periods of their lives they occupy very different amounts of space; but the question at present is whether they occupy more or less space under the actions of contraction and dilatation, respectively, at very short intervals of time, as, *e.g.*, iron does under the action of heat and cold). Usually, when an organised body, as the earth-worm, for example, shrinks in one direction, say its longitudinal direction, or in its length, it swells or expands or dilates in another direction, in its width, for instance; so that the space it occupies at any two shortly separated intervals of time is nearly if not quite the same. This property it probably owes to the water which always forms a large proportion of the structure of organised bodies, water being for practical purposes incompressible. A fact of this kind seems to have greatly impressed early inquirers into the constitution of nature, one of them, as is well known, having held that water (or moisture as we now term it), is the ultimate element from which all things are formed and to which they all return. Certainly without moisture life, as we know it, does not occur. The proximate causes, then, being swelling and shrinking (and the ancients knew

them under a great many different names, especially the Greeks, see Chap. VI.—the Latins called them *strictum et laxum*), it appears that these so-called causes are not really causes so much as states. These two causes or states have this peculiarity when manifested in organised bodies, that, whichever of them appears first in the animal economy, is always followed by its opposite; shrinking by swelling and swelling by shrinking, contraction by dilatation and dilatation by contraction. If a muscular fibre, for instance, contracts, it in course of time expands, and, after expanding, it again contracts. These properties depend on the constitution of the organised body, or on the constitution of the part of it under consideration; but we shall not be betrayed into the fallacy of supposing that contraction is the cause of swelling, although it always precedes it, nor yet that swelling in turn is the cause of contraction for the same reason, if we remember that day always precedes night and night day, although day is not the cause of night nor night the cause of day. These offer also a good instance of the fact that invariable succession is not the same thing as causation. Nevertheless, when we find one thing invariably preceding another, we may assume that there is some causal nexus, although it almost always takes the form that the two phenomena are successive effects of a common cause, although they are not cause and effect of one another. Day and night, for example, although

they are not cause and effect of one another, are successive effects of a common cause, namely, the rotation of the earth on its axis, and its behaviour to the sun. So the succession of swelling on shrinking and *vice versa*, may be considered best as successive effects of a common cause, namely, the constitution of the tissues of organised bodies.

There is another phase of the relation between swelling and shrinking which should not be overlooked, for failure to appreciate its significance has led to the greatest confusion in medicine, and is probably doing so to-day, notwithstanding all the advance which is alleged to have been made in the art. It is this. The structure of many parts of organised bodies, of the arteries, for example, which generally convey the aerated or arterial blood; of the intestines and of other parts; is in two layers of contractile structures, one of which goes along the length of the structure, and the other across its width. These are called respectively the longitudinal and the transverse fibres (or circular, sometimes). Now it is evident that contraction of the longitudinal fibres must cause dilatation of the transverse fibres, and that, on the other hand, contraction of the transverse fibres must cause dilatation of the longitudinal. In other words, contraction in the length of a vessel, or shortening it, widens its width, while contraction or narrowing of its width lengthens its length. When it is added that these two sets of fibres may be under the control of two different sets of nerves, and that

not infrequently the one set of nerves stimulates its fibres to contraction immediately after the other set has ceased to stimulate its own, it is evident how complicated may be the actions resulting from the successive action of two sets of structures acting in the body. Shortly, the complication is apt to create this amount of confusion in the observations of an inquirer that (1) contraction appears to cause dilatation and dilatation appears to cause contraction, and (2) that stimulation of one set of nerves appears to have the same effect as absence of stimulation or loss of control by another. That is to say, in both of these directions, actions may appear to produce effects exactly the opposite of those which they do exert. It is a little away from our subject at the present moment, but perhaps an example may illustrate better than a statement what is wished to be conveyed; and I may say, therefore, that contraction of fibres may either contract or dilate the pupil of the eye, and that paralysis of one of its nerves of supply may cause the same dilatation as is produced by stimulation of another. This complication and difficulty also arises in the attempt to understand the actions of many other parts of the body, and may lead us astray in studying respiration, cardiac action, intestinal action, sleep, thinking, and possibly, indeed, any and all functions. Even a poor imagination may perceive how entirely opposite must be recommendations as to treatment according to the mode in which

various changes in function are produced, or (and this is where the difficulty comes in, in practice), according to the possibly differing views of different advisers as to their mode of production, Evidently if a condition is caused by paralysis or loss of power in one set of nerves, its treatment must be by attempting to strengthen the same; but if, on the other hand, it is caused by over-stimulation of another set, equally clear is it that the object of treatment ought to be to remove some of the excess of action. On the former view the action must be tonic, bracing, strengthening; on the latter it must be lowering, relaxing, weakening. There can be no doubt that much difficulty enters here into medical practice, but I should like to add that the difficulty is by no means confined to medicine; commercial, social and ethical relations presenting frequently the same difficulties.

Simple as they are, the discussion of the proximate causes or states has led us further than we expected, and has opened up a field much wider and more difficult than we supposed. The exciting causes will hardly detain us long. These are such influences as heat, wetness, dryness, calm, wind, storm, and so on; also organic micro-organisms, spores, germs and organic particles; and, lastly, violence of various kinds. The effects of the last are dealt with by the surgeon; those of the others by the physician for the most part. It is very difficult to differentiate by exclusive characteristics the exciting causes from the predisposing, with

which I must deal directly; but probably as good a means of differentiation as any would be to say that while the exciting causes act once or seldom, the predisposing act often or for a long period of time. An attempt to distinguish the one set of causes from the other shews that causes which are considered to be exciting from one point of view may become predisposing from another. For instance, getting one's feet wet on a given occasion, or being in a wind, may be the exciting cause of an attack of inflammation of the lungs or of bronchitis; but being more or less continually exposed to the effects of damp or draughts may act as a predisposing cause to some long-continued illness.

The predisposing causes of disease are for the most part the relations of the body to air, to food, and to exercises, with perhaps the influences of anxiety and of heredity added. I shall hope to say something about the last later, and shall dismiss it for the present with the remark that as a cause of disease (though not of organisation, which is greatly determined by it) I believe its influence has been greatly exaggerated both by medical men and by the general public. And I go on to say that of the other predisposing causes of disease by far the most important is the relations of the body to food. This statement is really the main thesis of this essay. This thesis may be analysed into the following statements. First, if human beings were to arrange properly the quantity and quality of the food which they take, and also the times at which

they take it, they would suffer much less from disease and illness than they do now. Second, even after disease has occurred or has broken out, the best means to combat it and to restore the body to health is by an alteration in the diet. From which it follows, thirdly, that the best and most important means to keep a man in the health which by the supposition he has re-attained, is to prescribe for him a proper amount and quality of food, and suitable times of taking it for the future.

A great Latin medical writer, it is well known, divided methods of treatment into three divisions, treatment by diet (*victus*), by medicines (*medicamenta*), and by surgery (*manus*). And he might have added, I venture to suggest, that, with the exception of a few diseased conditions which *must* be treated by surgery, those diseases which cannot be treated by diet or by diet combined with exercises, which for the moment he forgot, can hardly be efficiently treated at all.

These positions must not be understood. It is not contended that the relations of the body to food are the only causes of disease. It is most freely admitted that there are other causes. No sensible man would deny or would wish to deny that the relation of the body to air or respiration is a very important cause of health on the one hand, and of disease on the other. Nor would he fail to attribute an important influence to the effects of exercise on the body. Among the exciting causes of disease, again, long exposure to cold or to heat,

or to fatigue, or to anxiety, as to many of the other exciting causes, must and does exert a very powerful influence for good or evil on health or disease. But after all these admissions are made, and due allowance made for the undoubted effect of these other causes in affecting the body for good and ill, the statement still seems to the writer to hold good that by far the most potent cause of disease, the cause compared with which all the rest pale into insignificance, is the predisposing cause of improper food supply, either as to quality or quantity, or both. How greatly this cause preponderates it is difficult to say, but the writer is of opinion, for reasons which will appear abundantly in the sequel, that it would probably be no exaggeration to say that proper or improper relations between the body and food are as great and important causes of health and disease as all other causes put together. The precise proportion, however, by which this cause preponderates over the others is immaterial to the argument, which alleges, on the other hand, that it is by far the most important cause, but by no means that it is the only one. A man may undoubtedly be worried to death, may be broken down by sorrow and anxiety, and his life and spirit worn down by this cause, however properly he may manage his digestion and food arrangements. And, on the other hand, so may he be chilled or destroyed by excessive and long continued cold or heat, or exposure, or overwork.

This will probably be the best place in which to introduce some ideas which we must consider sooner or later, regarding the division of diseases into *acute* and *chronic*. Under the action of such an exciting cause as exposure to cold, or wetness, or wind, a person is very apt to have a feverish attack, or an attack of inflammation, as it is called, in some organ or part. When this happens the temperature usually is found to rise above the normal standard of 98·4° F. (37° C.), and the pulse is generally found beating at a higher rate than the 60-90 beats a minute, which we laid down as the usual limits of health. The increase of the pulse-rate, and the elevation of the temperature are the signs, or some of them, by which nowadays we measure the acuteness or severity of an illness. Formerly it was not so. The ancient physicians did not measure temperature, although they put their hands on patients' bodies to feel if they were hot; nor do they seem to have counted the pulse, although they examined the pulse carefully, and attached great importance to its changes, and this notwithstanding that they did not realise or understand the circulation of the blood and lymph. The severity or acuteness of a disease seems to depend on two things; first, the amount or quantity of the exciting cause, the amount of the cold, or wind, or wetness, etc., to which a person may be exposed; and second, the strength or weakness of the patient. We should now say that the severity of an illness or its acuteness, as well as the length of

its duration, are proportional to the quantity of the cause in action, and that it is inversely as the resistance of the person acted upon, or (what comes to the same thing) proportional to his weakness or his predisposition to disease. An acute disease is generally a rather short-lived one, while milder diseases are generally longer in duration; and hence it is customary to classify diseases into *acute* or *severe* on the one hand, and into *chronic* on the other. It is, however, many years since I have been attempting to draw attention to the fact which I must emphasize again, that *acute* and *chronic* are not logical opposites. *Acute*, in fact, is opposed, not to *chronic*, but to *mild*; and between *acute* and *mild* there may be, and often is, inserted an intermediate group or class of diseases, viz., the *sub-acute*; while beyond the *acute* diseases it is not infrequently necessary to distinguish those which are extremely severe, or *per-acute*,* as they have been termed. *Chronic*, on the other hand, is the opposite, not of *acute*, but of *short*. The division of diseases into *acute* and *chronic* was introduced by Asclepiades of Prusa, who, after he went to Rome, became the medical adviser of Cicero. He was not a doctor, properly speaking, at all (of the rank, for instance, of Celsus or Galen) but a philosopher or dialectician. His contribution to medicine, of this very division of diseases into *acute* and *chronic*, as also his distinction between

* The term *ultra-acute*, suggested recently, seems to have been proposed in ignorance of the existence of the much better and time-honoured term *per-acute*.

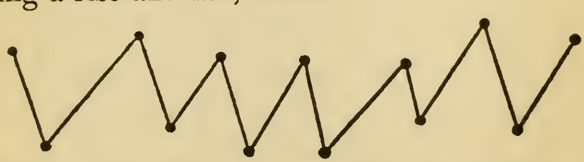
category and *prosagory*, as well as his ideas regarding the ὄγκοι (Latin, *Unci*) or hooked atoms, a possible anticipation of the modern doctrine of microbes, and of the part they may be supposed to play in the causation of disease, all bear the impress rather of the philosopher than the practical physician. His division of diseases into *acute* and *chronic*, however, has been most unfortunate, and offers one of the most interesting instances of an alleged reform being really a retrogression, because it can be shewn that Hippocrates, hundreds of years before, had spoken of diseases as being *mild* (πραεῖς) and *severe* or *acute* (ὀξεῖς) and had also spoken of diseases as being *short* (βραχεῖς or ὀλιγοχρόνιοι) and *long* (πολυχρόνιοι or χρόνιοι or μακροὶ), so keeping things which were distinct in fact, clear and distinct in his mind and in language. And yet the so-called reform of Asclepiades has held its own ever since his time, displacing the more logical ideas of Hippocrates in this particular. In Latin the word *lentus* is often used of the chronic diseases. As *lentus* means *tenacious*, hence *slow, sluggish*, hence *lasting*, and as long diseases are generally somewhat mild, there has been no relief to the ambiguity, derived from the use of Latin terminology, such *e.g.*, as the use of the word *mitis* for *mild* or *slight* would have given us.

Chronic means long continued, and the term has been applied to any diseased state of the organism lasting for twenty-eight days or longer. To lengthen its application or definition to diseases

lasting three months would, I think, be better, because a few cases of what is called acute disease last as long as three months (some even more), *e.g.*, some cases of typhoid fever, which certainly go on occasionally for six, nine, or twelve weeks, or longer; or cases of rheumatic fever. No doubt *chronic* diseases are generally *mild*, but frequently they are not so, being *sub-acute* or *acute*, or even *per-acute* (in accordance with the definitions immediately to be given to these terms), while more often they are alternately *mild* at some periods of their course, and *acute* or *sub-acute.* or *per-acute* at others. Or they may be intermittent or recurrent; that is, they may cease entirely for a time, during which the patient gets quite well, and then they may recur. Again, disease, without entirely ceasing, may improve considerably, becoming milder for a time, and then become more severe again; in which case it is termed *remittent* rather than *intermittent.* But when disease settles down on a patient, or, as it may be better expressed, when the patient is always ill, for as long (I propose) as not less than three months at a time, then his disease is termed *chronic.* Even in this last case, however, when patients are always ill, they are not equally ill at all times, for, if the case is watched closely, it will always be found that the patient is worse at some times and not so ill again at others. We may otherwise express this state by saying that a patient is recurrently or intermittently ill, and, by and bye, that he is remittently ill, before he becomes

chronically ill. The cause of this alternation between being better and worse or between being ill and again not so ill, is a very interesting one. I believe it depends on the manner of the rotation of the earth on which we live, and its behaviour to the sun ; for that arrangement implies that our earth, in revolving on its axis, projects now this side and then that to the sun, so that in the former case it is day on one part of the planet and night on the other, and in the course of twelve hours or so these relations are reversed. Now, when it is day, on a certain part of the planet, all organic things on that part are shrunk and ready for action like a spring compressed which is ready to uncoil as the day goes on ; till, when night comes on, they are swollen like an uncoiled spring and go to sleep, during which time the spring becomes shrunk again, or compressed, as it were, and is ready on another day to translate its uncoiling into the doing of physical and mental work. The effect of this arrangement is to produce the intermittent work or action as distinguished from constant or continuous action, which is so characteristic of everything that occurs on this planet, that all action is intermittent and even what seems to be continuous action, translates itself always into a rapid succession of intermittent actions. Hence the alterations of pulse-rate and temperature which we observe, not only in disease but even in health to some extent ; and hence also those alternations which invariably occur in all the

relations of man to his environment, traceable, as they are, in politics, commerce, and philosophy, and which determine the behaviour even of the ultimate elements of protoplasm, so that they are seen, whether by the naked eye or under the microscope, to be in constant alternation of swelling on the one hand and shrinking on the other. Even inorganic phenomena manifest this characteristic, and the very tide rises and falls in an intermittent and not in a continuous way, alternately rising and falling as it flows, and falling and rising as it ebbs. It is an interesting speculation, and one not wholly unpractical, to inquire what would be the effect on life and on phenomena on a planet which should behave differently to the sun, as the moon, for instance, behaves to the earth, rotating on its axis, indeed, but in such a way as to keep always the same face to the sun. Obviously there would be everlasting day on one side of such a planet, and everlasting night on the opposite one; and the effect on life and on every action occurring on that planet would probably be very different indeed from what occurs on our earth. It seems as if the diagrammatic representation of life in this latter case would be, not as with us, a zig-zag line representing a rise and fall, thus—



but would be, on the contrary, represented by a

point which widens into a line, the line widening to its broadest place, after which it would begin to become narrower and narrower till it ended in a point again, thus—

and probably, the action of life on such a planet would be continuous, and not intermittent as on ours, beginning with slight actions, which would become stronger and stronger, till they culminated at the central point in the double cone, after which they would generally diminish, but still in a continuously lessening manner, till they finally ended in a point as they began.

But however this may be, and to return to our consideration of *acute* and *chronic*, when we reflect that *chronic* diseases are severe enough, or *acute* enough to kill patients, we seem to realise better the confusion which exists in our minds in thinking about them. *Chronic* being, then, the opposite, not of *acute*, but of *short* or *brief*, we can define a *short*, or *brief*, or brachy-chronic disease (*νοῦσος βραχυχρόνιος*, or *morbus brevis*), as one lasting, I would suggest, not longer than three months. If it lasted longer than this, it would be chronic (*νοῦσος χρόνιος*, or *πολυχρόνιος*, or *μακρὸς*, *morbus longus* or *chronicus*). Then a rather short disease (*νοῦσος βραχυχρονιώτερα*, or *morbus brevior*) would be defined as one lasting 14 days or so; while a very

short disease (*νούσος βραχυχρονιωτατα*, or *morbus brevissimus*), would be one which lasted from two to seven days. A disease lasting one day has often been termed ephemeral (*νούσος ἐΦημέριος*, or *morbus diurnus*). Diseases which recurred, on the other hand, at daily intervals, or at intervals of two or of three days, were called, respectively, quotidian, tertian, and quartan diseases. In the opinion of Asclepiades of Prusa, the differences in the length of the duration of these last diseases were determined by differences in the sizes of the *ὄγκοι*, or *unci*, or hooked atoms co-existing with them, a doctrine which, as has been said, may be considered anticipatory 1800 years ago, of the germ theory of disease, of which we have heard so much of late years.

Acute or severe diseases (*νούσοι ὀξεῖς*, or *morbi acuti*), on the other hand, may be defined as those in which the pulse-rate reaches, say, 120 or 130 a minute, and the temperature, say 102·5° to 104° F.

Peracute diseases (*νούσοι ὀξυτάται* or *ὑπεροξεῖς*, or *morbi peracuti*), are those in which the pulse reaches 130 a minute or over, and the temperature is over 104° F. (Sometimes the temperature does not rise in these diseases, being kept down by failure or inability of the body to react, or by too great shock.) In mild diseases (*νούσοι πραεῖς*, or *morbi mites* or *lenti*), the pulse is not more rapid than about 100 a minute, and the temperature not above 99·5° or 100° F.

Acuteness of disease, therefore, depending on its severity and not on the length of its duration,

its opposite is mildness, not length of duration. And a subdivision might be made, and often is made, between acute and mild diseases, of the subacute diseases (*νούσοι ὑποξεῖς*, or *morbi subacuti*), in which the pulse-rate will be about 110 to 120, and the temperature between 100° and 102° F., or so.

Diseases which are *severe*, or *acute*, or *per-acute*, are generally also *short*, because the human body cannot bear very severe suffering for any great length of time without either relief or death; and this has no doubt been the chief determining consideration which has led in men's minds to the confusion in classification into the *acute* and *chronic* diseases. I said that Hippocrates was much clearer in his ideas on this subject, and in his use of language regarding it, than was his successor Asclepiades of Prusa; and this is true. But even Hippocrates was not always quite clear on the point, and in one of his aphorisms, he says, that acute diseases can be judged or determined (by the observer) in fourteen days—a most unfortunate expression, as it happens, since it has been supposed to mean that he defined acute diseases as those that last fourteen days, so justifying that confusion between severity and length of duration in diseases which has obtained for so long a time. The words of Hippocrates are, *τὰ ὀξέα τῶν νοσημάτων ἐν τεσσαρεσκαίδεκα ἡμέρῃσιν κρίνεται*. Here a good deal may turn on the meaning we attach to the word *κρίνεται*. It seems, however, clear that

what Hippocrates did mean was, not that acute diseases were those that last fourteen days, but this, that in fourteen days the observer will generally be able to determine what sort of disease his patient is suffering from, and what the issue is likely to be; whether the illness is likely to be long or short, and also whether it is likely to be fatal or otherwise. These propositions are true enough; and, in fact, we can usually form opinions on these points before even fourteen days have elapsed from the commencement of the illness we may be treating. But while we must admit that Hippocrates was not always quite clear in his language regarding acute and mild diseases on the one hand, nor as to short and long ones on the other (see, for instance, a passage in *περὶ κρισίων*, where he says that diseases *κρίνονται* up till the sixtieth day, after which they become chronic), his confusion was never so dark as that of the so-called reformer who came after him, and who misled his successors in turn. The remarks made, and the descriptions suggested for *mild* and *acute*, and *long* and *short* conditions will, it is hoped, show how a disease may, in its course, be now acute, and then mild, and again sub-acute, and that these conditions have no necessary connection with the length of its duration; and they will, I hope, therefore, have proved that the division of diseases into *acute* and *chronic* is not a logical one, and that it ought, therefore, to be superseded by one more in accordance with the order of nature.

I hope to be able to adduce evidence which offers a reasonable amount of proof for the correctness of the view I put forward that wrong feeding is the chief or paramount cause of disease, both *acute* or *mild* on the one hand, and *chronic* or *brief* on the other. This evidence should be capable of receiving corroboration in three directions. First, it is easy to shew that all diseases are most easily treated by alteration of the diet. This statement will hardly be disputed. By common consent it is true, and common practice bears it out. In fact, alteration of the diet, and almost always, or at least very frequently, restriction of the diet, is nature's method of cure. Certainly in all acute diseases this is so much the case that restriction of the diet, and often, indeed, entire abstinence from food, is the treatment adopted by nature. In these diseases she flings the patient into bed with a furred tongue, or dry and brown tongue and mouth, with complete inability to take any food, and very often also complete refusal to take it, or to digest and assimilate it, if, in spite of her clear indications, it *is* taken. Second, it can be shewn in most cases that, this disinclination to take food persisting for a time, the patient recovers, and remains well so long as he continues to take proper quantities of food at proper times. Thirdly, it will be shewn in so many cases as to raise a reasonable presumption that it is the rule in the body, that disease is re-induced in those cases in which return is made again to those methods of feeding which made the patient ill

before. I do not myself believe that the fevers which are generally supposed to be caused by bad or vitiated air offer any exception to this statement. I think that the chief predisposing cause of them, as, *e.g.*, of measles, scarlatina, diphtheria, typhoid fever, influenza, and the rest, is improper feeding, and chiefly over-feeding. It is true that it is difficult to shew this in the case of the continued fevers, because, as a rule, these diseases occur only once in life. Although they seem to me to occur oftener than once in life, more frequently than is commonly believed—how often do we find patients having more than one attack of measles or scarlet fever (scarlatina), for instance—I admit that recurrence is exceptional, and that as a rule they occur only once. This is not true, however, of influenza, which is properly classed among the fevers. The definition which I should offer of the zymotic diseases (*ξυμόω*, = I leaven or make to ferment), or of the fevers (*ferveo* = I burn) is that they are febrile diseases known or believed to be associated with the presence and growth of micro-organisms in the blood or tissues of the body. Inflammations, on the other hand (*flamma* = a flame), might be defined as febrile diseases not known to be associated with the presence and growth of such micro-organisms. Some inflammations, it is true, are believed to be associated, or are suspected to be associated, with the presence and growth of micro-organisms in the body. Some forms of pneumonia, for instance, or inflammation of the lungs, are

now known to be so associated; while lately, as regards acute rheumatism, or rheumatic fever, as it is more frequently called, the same suggestion has been made, or the same suspicion entertained. Such inflammations are often termed specific inflammations, and of them it may be said that medical opinion is tending towards including and classifying them among the fevers. But at present we may say that the specific inflammations form a group of diseases between the fevers and the simple inflammations. As regards these last diseases, indeed, the simple inflammations, I should not be greatly surprised if it should be found before long, as the knowledge of the life history of the numerous micro-organisms becomes more thoroughly known, that more and more of the inflammations will be found to be associated with the growth in the body of some forms of micro-organisms. In this case the distinction between the fevers and the inflammations would tend to become obliterated. Up to the present time, however, this association of the growth of micro-organisms has not been shewn in the case of the simple inflammations, such, for instance, as bronchitis, simple broncho-pneumonia, or in such affections as inflammation of the heart, stomach, liver or kidneys, and, therefore, the distinction between the fevers and the inflammations still holds good. And further, the clinical distinction between the fevers and the inflammations, that the former appear only once in life while the latter may and often do occur again and again,

holds true in the main. It is, therefore, very difficult to prove the third part of our argument in the case of the fevers, viz., that a recurrence to those modes of feeding which induced them at first will re-induce them. In the case of influenza, however, this difficulty does not occur. Influenza belongs to the fevers, because the micro-organism associated with its occurrence in the body, has been cultivated and developed again and again. I have no doubt at all that the chief predisposing cause of influenza is wrong feeding and too frequent feeding; and the kind of evidence which I offer for the holding of this opinion is represented by cases like the following. A man about 38 years of age consulted me for frequently recurring attacks of broncho-pneumonia, or severe feverish colds, as they are popularly termed, and mentioned also that he had had influenza in three consecutive years. He offered to go to South Africa or anywhere where I might think he would have a better chance of living a healthy life, if only he could be rid of his crippling attacks of illness. I told him that he might go to South Africa if he liked, but that, in my opinion, he could get well in Bradford if he would take a little trouble. Further, I said that if he did go to South Africa, and if he got rid of his severe and recurring colds there, and if he did not in the meantime alter his ways, and particularly his food habits (there was no suggestion of alcoholism in the case, the man being, if not a teetotaller, very temperate), the colds would infallibly be replaced

by some other form of illness, rheumatism, *e.g.*, or gout, or perhaps by the onset of some disease generally attributed to the South African climate. In short, I told him that it mattered far less *where* he lived than *how*. I advised him to cut down his food, which he evidently was not assimilatiug, and in particular to take two meals daily in place of three, and I suggested that he should diminish his bread. The effect of these changes has been most beneficial. After one other broncho-pneumonic attack, whose effect was most salutary to him, as it helped to rid his body of the accumulation of effete and unassimilated stuff which was stored up within it, he has remained now free of his old "colds" for about four years; and he has not had influenza since. As these advantages were confidently predicted to him as consequences which would ensue on the alteration of his food habits, and as they have accrued to him accordingly on his effecting that alteration, I have no doubt that his colds and his influenzas were dependent on the wrong feeding to which he used to be subject. If this is so, we have a recurring inflammation, and also a fever, put an end to by alteration of the diet, and particularly by restriction of the diet. If the man has any doubt of the sequence of cause and effect in the case, he has only to return to his former way of living; which, if he did, I have no doubt that in a few weeks he would get another attack of broncho-pneumonia, and, no doubt, after some time longer, would again find himself suffering from influenza.

But I do not recommend that this experiment should be made, although I have no doubt (neither has my patient, to whom the whole matter has been a great surprise) what the result would be if it were. The same experience occurred to me in treating a patient who had suffered for many years from a succession of bronchitic and asthmatic attacks. He had lived in the West Indies for some years, some thirty years before the time to which I am now referring. While there he had suffered greatly from malarial fever, "fever and ague," as it was termed there, and so profoundly had his organism become modified that he never took one of his many and continually recurring colds without suffering also from an attack of ague. Being advised to restrict his diet in order to get rid of his bronchitis and asthma, and having done as he was advised, he made a complete recovery, not only from the bronchitis and asthma, but also from chronic rheumatism, from which he used to suffer, as well as recurring attacks of herpes, or watery blisters, on his lips, mouth and tongue. And, still further, to the surprise both of the doctor and of the patient, he has not now had an attack of ague for over ten years. It really does seem marvellous that so marked and so gratifying a result as this could occur after about fifty years of age, and after the body had suffered for so long a time as thirty years; but so it was. That patient would be very ill-advised if he were to carry scepticism as to whether his recovery depended on the treatment, so

far as to have recourse again to the mode of feeding which made him so ill before. Nevertheless, if he were to do so, I have no doubt that he would soon again begin to suffer from herpes of the lips, from rheumatism, from bronchitis and asthma, and, by and bye, even from recurring ague. But as malarial fever and ague is technically a fever, here is another case in which an alteration of the food habits and restriction of the diet have cured and prevented the recurrence both of inflammations and of fever.

I can imagine some reader here saying to himself, why was not quinine administered to this patient "to break the periodicity of his attacks?" It is well known, I hear him mentally adding, that quinine is specific, and a sovereign remedy in intermittent fever. Alas for the suggestion—quinine *was* administered, and that in large doses. Often had he been cinchonised while in the West Indies by the quinine which he took. On one occasion he took 120 grains of quinine in three weeks, but without any noticeable effect on his intermittent fever. Then he was recommended to take arsenic, which did him good for a time, but the attacks recurred, because the causes of the attacks, viz., improper feeding, continued to act. It is easy to see this now, but no one had the ghost of an idea of it then. It had never occurred to anyone interested in the case that food had anything to do with it, and the illness was attributed wholly to the West Indian climate. Nevertheless, it is plain now, on looking back, that

it had everything to do with it, and that a cause acting constantly, or at least at very short intervals of time (three or four or more times a day), shewed its effects in intermittent or periodic attacks of ague, separated from one another by longer intervals of time. This is in perfect accordance with the law elsewhere stated that constant causes acting on the economy shew their influence, not in constant, but in periodic effects. If the reader is interested in this case, I will mention one or two other incidents in it. After quinine had failed to improve the patient at all, and when, notwithstanding the administration of arsenic, the attacks still recurred, much benefit was obtained by an early morning shower-bath, given in the primitive way of emptying a large calabash full of water, left in the open air all night, over the head and shoulders and back of the patient. It was guilelessly believed that the water was more efficacious for having been left out all night; but obviously it was the cold shower bath which did the good, and a bath fitted up in the house, if there had been a bath-room in the house, would have had the same or more effect. By and bye, however, this also lost its effect, and the malarial attacks returned, no noticeable and permanent improvement being apparent until the patient left the tropical country for a temperate one. Even then, however, and for 30 years afterwards, an attack of ague came on again with every cold. The only question remaining on one's mind now in respect of this case is the following :—

Would, it may be asked, a change of diet in the tropical country itself, given moderately sanitary conditions present, would such a change of diet, say two meals a day, to an extent of not more than 12 ozs. a day, without a change of climate, have cured the patient? For my part, I believe it would. Opinions will differ about this. It is a hypothetical question which can never be translated into terms of actual experiment for the given case, although there are no doubt now many thousands of similar cases going on among Europeans living in tropical countries, to whom such a narrative may be of the very greatest interest. If my opinion is sound, they might save themselves much suffering, much trouble, and much money. The development of rheumatism, bronchitis, and asthma, in the temperate climate, shewed how superficial was the change that relieved the patient (though it did not cure him) of the recurring malarial attacks, and shews also, to all who wish to see it, that the chief cause of all the ailments, tropical and temperate, was improper feeding. I, at least, have no doubt that this man ate his diseases, that he ate his malaria, his rheumatism, his bronchitis, and his asthma, of all of which, even after 50 years of age (that is the surprising thing) he was able to get rid on changing his diet. No doubt he was bitten by mosquitoes, but the question is, would their bites have done him much harm if he had been properly fed? And at any rate they did not bite him for the 30 years after he left the tropics, and yet he

continued to have malaria for that length of time, and I do not doubt would have gone on having it still (if he had lived) if he had not altered his diet. Neither have I the slightest doubt that the other patient got rid of his broncho-pneumonias and his influenzas by the same means.

In the usual case, when one of the continued fevers is taken, scarlet fever, for instance, which does not as a rule occur more than once in life, it is difficult, as I have said, to see the effects of the continuance of long acting causes, like prolonged improper feeding. What usually happens is, either that some inflammation, or an attack of some other fever, follows as the effect of long acting or of the frequently repeated causes. The person who has had scarlet fever before does not, as a rule, take scarlet fever again (he does so sometimes, however), but he has, say, an attack of diphtheria, or of measles, perhaps, or typhoid fever, or perhaps a severe "feverish cold"; and so the causal nexus is not seen. As the disease is called by a different name in each case, and as, further, it is attributed to such exciting causes as infection, exposure to cold, damp, fatigue, &c., the connection of each attack with the predisposing cause of improper feeding, and with the bad, unresisting state of body so induced, is not seen, or is overlooked. Yet all the while, the occurrence of the severe feverish cold, or of the measles, or of the broncho-pneumonia, was, or might have been, as much the effect of long-continued improper feeding, as the original

attack of scarlatina, or other so-called infectious fever was.

As was said in the last chapter, it is commonly believed that the fevers are caused, not by improper feeding, but by breathing bad or vitiated air; and on this doctrine the whole of the modern practice regarding the prevention of the fevers has been based. Having suggested before that I was not altogether satisfied with the truth of this doctrine, I now proceed to give some more of the reasons which lead me to disbelieve in its correctness. Attention to air and to supplying an abundance of pure air is an excellent measure, and has had a great influence in preventing the incidence of fevers, as it has had also in limiting their spread. In point of fact, it has diminished fevers by from 40 to 50 per cent. in the last forty or fifty years. But if bad air had been the whole of the cause, or the main part of the cause, of fevers, supplying good air would probably have diminished fevers by more than 40 or 50 per cent. When, for instance, the chief cause of bronchitis is known and is eliminated (except, of course, in those cases where organic change has gone so far as to have rendered recovery impossible), not only is the bronchitis cured, but its recurrence is prevented. If we had been acting on the whole of the cause of fevers we ought to have diminished them by 80 or 90 per cent. Besides this, under a free supply of good air, influenza (which is a fever, for the micro-organism associated with it is known and cultivated) has come

among the English people, and has annually attacked them since 1890 or so. It is plain, therefore, that bad air is not the main cause of influenza, although it may be a contributory cause, and that of an important character. Another reason for thinking that bad air has had too much importance attached to it as a cause of fevers is that during the past ten years no appreciable diminution in the incidence or mortality from fevers has been effected in England and Wales. This statement may appear extravagant to those who have had in their minds only the great reduction which has taken place in the incidence and mortality from the infectious fevers or the zymotic diseases during the last two generations. The following facts, therefore, on which this statement is made, will be of interest. In the five years, 1876-80, the death rate from zymotic disease in England and Wales was 3·823 per 1000; in 1881-5 it was 2·804 per 1000; in 1886-90 it was 2·502 per 1000; in 1891-5 it was 2·747 per 1000; and in 1896-99 it was 2·678 per 1000. It will be seen that the zymotic mortality was less in 1886-90, before influenza appeared, than it was in 1891-5 or 1896-99. About the time of the Crimean War, the death rate from the zymotic diseases was about 5·240 per 1000 per annum. Since that time the fall has been very marked; but in the last ten years there has been a slight increase instead of a diminution. It seems as if we had exhausted the benefit in this respect which we are likely to get from improved sanitation, so far,

that is, as improved air is concerned. In 1890 influenza caused 4523 deaths in England and Wales; in 1891, 16,686; and in 1895, 12,880 deaths. In 1896-7-8-9 and 1900 the numbers were 3753, 6088, 10,405, 12,417, and 16,245, respectively.

In attempting, therefore, to come to a conclusion regarding the causes of the fevers or zymotic diseases, these three reasons make us hesitate to accept the prevailing opinion that breathing bad or vitiated air is the most important. (1) The reduction of fever has only been from about 40 to 50 per cent. in about 50 years; (2) during the last ten years there has been no reduction in the mortality from fevers at all; and (3) during the last ten years influenza has come, apparently to stay. No intermission has taken place in our sanitary efforts during that period. On the contrary, our sanitary authorities are carrying out, with the loyal co-operation of the people, and even, it may be said, under instructions from them, vast sanitary works, whose total cost must be little, if at all, short of the amount of the National Debt of this country. The results are not satisfactory—certainly not so satisfactory as they ought to be; and my suggestion is, Has not a main part of the cause of these epidemic diseases been overlooked? I suggest that this is so, and that what has been overlooked is the increased frequency of meals, which is so characteristic of our time. The three meals a day which used to be sufficient for us are no longer so. Even

our paupers, breakfasting at 8-0 or 8-30, have a lunch of bread and cheese, men and women alike, at 10-30 or 11 a.m., and dine at 12-30. It is quite impossible that the food taken at 8-0 a.m. can be digested at 10-30 or 11-0; and it is equally impossible that that taken at 10-30 can be digested at 12-30 or 1-0 p.m. There is no time for it. Even when breakfast is taken at 8-0, digestion is so slow in many cases that the food is not digested—frequently it has not left the stomach—at 12-30 or 1-0. But even if this is not very frequently the case, and if, therefore, it is reasonable to take a second meal four or four-and-a-half hours after the first, it is quite certain that food taken at 10-30 is not digested at 12-30 or 1-0. There is no time for it to be so. Nothing, therefore, could be less wise, physiologically, than the interpolation of this supernumerary meal, and the interpolation is a very striking instance of carrying out into practice the unsound doctrines which are current on this subject at the present time. One wonders where the physiological advisers of the Local Government Board were, or what they were thinking about, when this course was adopted at the public expense. The same course in principle has been followed in arranging the diet of our sailors in the Navy, although there is no proof that they were under-fed or too seldom fed before. One must only hope that their fighting powers will not be impaired by the new *regime,* and that there will be no more sickness among them than before. The experience of the

Boer War in South Africa was to the effect that our men, after having a cup of coffee and a biscuit about four in the morning, marched or rode thereafter till about four in the afternoon, when they halted and had a meal, that completing the tale of their food for 24 hours, and that on this diet they were active and well. These men were in the early flush of life and activity, and were also doing active work. If they could thrive on these food arrangements, how unnecessary must it be considered to allow more numerous meals to paupers leading for the most part an inactive or little active life. It is no reproach to the paupers that they should be inactive at the age which many of them have reached, but still it must be remembered that in inactivity less food is required than when persons are younger and performing more active duties. The experience of the Roman soldiers, who were called upon to do anything and to go anywhere, was very similar to that of our men in South Africa, for de Quincey tells us that they ate once a day. The habit of taking food once a day must have been a recognised one among some of the population of imperial Rome, for we find Celsus recommending, in the second century of the Christian era, the taking of two meals rather than one. *"Bis die potius,"* he says, *"quam semel cibum capere."* Still we should hardly be justified in inferring that it was anything like a universal custom in imperial Rome to eat once a day, or even twice, for we find from Athenæus, perhaps 50-100 years after the time

of Celsus, that the custom had quietly altered, and that it had become usual to take, as we are doing now, four or five meals a day. These were named ἀκράτισμα (early breakfast), ἄριστον (perhaps similar to what we call lunch, although the term is often applied to breakfast), ἑσπέρισμα (an afternoon meal), and δεῖπνον (supper). Under the influence of the spread of wealth, just as is happening in this country now (it is not, it seems to me, the poverty of England, it is its wealth which is our danger—it is, I mean, the foolish use we are making of our wealth and its bad distribution), food customs gradually altered in the ancient world. The time had long gone by, and, more important still, the human outlook had greatly altered since the time when, about 500 years before Christ, Hippocrates describes the practice of what was then the Ancient Medicine, and in reference to which he gives his own personal opinion that it is immaterial whether a man eats once a day or twice. We seem to have it suggested to us that at that time it was a common custom to do either one or the other. In other places in his writings, it is true, Hippocrates refers to customs involving the taking of more frequent meals, so that there was evidently no fixed rule among the Greek peoples among whom he practised. It is, however, interesting to note that those among ourselves who have been so profoundly shocked by the suggestion recently resuscitated among us, that it is often wise to abstain from the meal called breakfast (or rather postpone it, for that

is the meaning of taking the first meal at 12 or one o'clock—it is a postponement, not an omission of the breaking-of-the-fast), might, if they chose to think or to read a little, find that many thousands of the inhabitants of this planet have lived long and happily and usefully on even one meal a day, as well as on two. A piece of advice offered by Hippocrates to his countrymen, some time before they successfully opposed the Persian invasion of Greece by Xerxes, was to the effect that, if a man found himself not very well, he should abstain from breakfast, at least if the weather was hot. At this season, the physiological instinct of the physician told him from his clinical experience, although he had no chemical ideas to guide him, chemistry up to that time being only an alchemist's dream, that a man required less food then than he did in cold weather. Let the man, says Hippocrates, go without his breakfast (*ἀνὰριστος μὲν διαγέτω*), if it is summer time.

It is no part of the physician's duty to trench on the sphere of the moralist or spiritual adviser; but if a proper view of the processes of nature is taken by each in his own sphere, it is remarkable how similar is its course seen to be in all departments; and it is impossible to avoid being struck by the observation that, as great and numerous banquets and other forms of sensuous indulgence have become prominent, so have the nations pursuing that course of conduct decayed. What has happened before will certainly

occur again; and any nation which pursues pleasure, wealth, and sensual gratification too keenly, will certainly begin to decay and perish. In this respect, as in every other, moderation is the only fixed and the only unfailing rule. If the Roman habit came to be to take *jentaculum*, *prandium*, and *cœnam*, breakfast, dinner, and supper, as we now call them, we know that at former times, as Celsus hints to us, the Romans lived on fewer meals than three; and, unfortunately, we also know that, as wealth increased among them, so did luxury, and that luxury translated itself among them as one of its forms into the taking of more and more meals, of a more and more elaborate sort, and that the habits accompanying this mode of living coincided in the most marked manner with national decay. It has often seemed as if a very striking parallel might be drawn between the Rome of, say, 200 to 350 A.D., and the London of to-day. "Rome," says Athenaeus, writing probably about 250 A.D., "may fairly be called the nation of the world. And he will not be far out who pronounces the city of the Romans an epitome of the whole earth; for in it you may see every other city arranged collectively, and many also separately; for instance, there you may see the golden city of the Alexandrians, the beautiful metropolis of Antioch, the surpassing beauty of Nicomadia, and besides all these, that most glorious of all the cities which Jupiter has ever displayed, I mean Athens. And not only one

day, but all the days in an entire year, would be too short for a man who should attempt to enumerate all the cities which might be enumerated as discernible in that uranopolis of the Romans, the city of Rome, so numerous are they. For, indeed, some entire nations are settled there, as the Cappadocians, the Scythians, the people of Pontus, and many others." How can one fail to be struck by the similarity between the state of things here described as obtaining in Rome at that time, and that obtaining in London to-day? Substitute modern cities arranged collectively and separately for the ancient ones named by Athènaeus; substitute the thousands of Frenchmen in London from gay and volatile Paris, the thousands of Italians from ancient and stately Rome, the thousands of Germans, harsh of speech and full of enterprise, from our neighbouring Teutonic land, and we have a picture of the uranopolis of London very like the uranopolis of ancient Rome. And if we add to the picture the Jews in England, probably not less numerous than the whole of the race resident in Palestine in her most palmy days, and picture, besides, the representatives of Australia, Canada, and New Zealand, the refined and subtle Indian, and the patient and artistic Chinese, not to speak of representatives of the still uncivilised South African peoples, our imagination must be poor and unmovable indeed if we do not see a likeness between ancient Rome and the modern commercial

capital of the world. And if, in spite of ourselves, we proceed to inquire whether the causes that subverted the one are not again in action, tending to dethrone the other, and if we feel ourselves compelled to reflect that even in despite of Christianity, the absence of slavery, at least in name, and the existence of free political institutions; the spread of luxury and the unrestrained worship of wealth and pleasure are becoming very apparent in the London of to-day, we are compelled to pause and consider. There is no doubt plenty of grit in England yet—there are more than seven thousand men who have not bowed the knee to Baal in this matter—but the general tendency towards extravagance and luxury, who can fail to be struck by it, or who can refrain from asking to what it may lead? *Panem et Circenses*, said the Roman proletariat; food and amusement for nothing. Are not these parallelled, if faintly, by heavy receipts of gate money at sports, by demands for houses without payment or equivalent contributions in labour, by demands for old age pensions towards which no contributions are to be made by the recipients, the while that charities are languishing for lack of support, while art and literature are being neglected, and education can be recommended and defended only for the commercial advantages which it is expected to bring?

The reader may here say to himself: well, but are not visitations of epidemic disease also well known sometimes to be due, not to over-feeding,

but to under-feeding? Not to plethora, but to starvation? Was not the relapsing fever epidemic in Ireland, in 1848, and the typhus fever that accompanied it, as also the Indian recent epidemic of plague, due to the failure of the potato crop in Ireland, and to the failure of the rice crop in India? Yes, I admit that they were, but I think that one or two qualifications ought to be made, which I will proceed to do directly. First of all, however, let us note this. I say that moderation is the only fixed and the only unfailing rule of living; and in the case of food supply to people living in towns, I attempt to more or less define moderation (an indefinable and indefinite thing, of course), as lying between 12 and 24 ozs. of ordinary diet of wholesome quality a day, taken preferably at two meals, eight hours or so apart from one another. I do not consider starvation as moderation. Starvation is as much below moderation as plethora and too frequent eating is above it. Neither is good. Neither, secondly, do I say that starvation cannot cause fever. I think it can, *especially if people have been too well fed before.* This is the first qualification which I think ought to be made when I admit that starvation also may cause fever. Because, if people were in a quite healthy state before, that is, if they had been moderately fed, neither by too much nor by too little, I believe that the effects of starvation would be not to cause fever, nor the feverish state, but to slowly wear them down with a slowly progressive diminution of

strength. Fever and feverishness are, I believe, re-actions from this condition, and are due, not so much to the starvation, as to the fact that for a long time previously their bodies and blood had been loaded with waste unassimilated materials derived from an excess of (even wholesome) food. The fever and feverishness are *occasioned* no doubt by the fasting, but not *caused* by it. This means, of course, when we come to analyse it, as has been before said, that the starvation is part of the cause of the fever, but that the chief part of the cause is the state that our bodies are in when the fast is compulsorily forced on us. So that I have come to accept the conclusion (I do so for myself, at least, which, if it does not prove my wisdom, proves, at least, my good faith), that if we cannot fast without fever, it is because we have been previously improperly fed; and in my own case I should accept the verdict, unquestioningly, that it was because for some considerable time previously I had been over-fed. But although admitting in this qualified sense that starvation may cause a visitation even of epidemic disease, this admission does not hinder me from asserting that, as a rule, epidemics are in fact caused far more frequently by over-feeding than by starvation.

There is, however, another and a more important qualification which I would make regarding the admission that starvation may cause epidemic disease, and that is this. It seems to me to have been too much overlooked. When crops

fail, so as to cause famine, they seldom fail utterly, but they are short *and bad.* The potato in Ireland, and the rice in India, were not only short, but they were also diseased. I think that shortage *and* badness of crops may well be admitted to account for the presence of epidemic disease among a people said or thought to be suffering from starvation pure and simple, when mere shortage might not have been sufficient to account for it.

These are in brief the reasons, or some of them, why I cannot accept or feel satisfied with the prevailing opinion that bad air is the chief cause of the fevers. I might corroborate this opinion by other well known facts. When a person who is taking too much alcohol makes up his mind to pull up and cease taking it, he frequently suffers very much at first, and for several weeks, after getting on to a better way of living. He institutes a fast from alcohol, and it distresses him greatly. Obviously, his distress is *occasioned* by his abstinence from alcohol, that is, his abstinence is a part of the cause of his distress, but the chief part of the cause is not the abstinence, but the fact that for a long time previously he was in the habit of taking too much. And if, on account of his distress, which is no doubt real enough, and not at all imaginary, he should throw up his good resolutions, and say that total abstinence did not suit him, he would not only be very unwise, and very unreasonable, but he would in addition be in the perilous way of running the risk of forfeiting

whatever chance he might have had of recovering his health, and of overcoming his bad habits. And so with the person who finds himself suffering from a fever, due, as he may suppose, to too little food. It is not the fast, but the previous over-feeding, from which he is really suffering, and which is the most potent cause of his attack. The reference to alcohol may perhaps justify my going on to say that while alcohol, in those who take too much of it, may be a cause, and a potent one, of epidemic disease, it seldom accounts for the onset of the Fevers in this country. These affections, measles, scarlatina, diphtheria, chicken pox, &c., are mainly rife among children, who, as a rule, take no alcohol at all. No doubt, however, epidemics, like influenza, typhus, small pox, &c., which are apt to attack older people, may to some extent be caused by the excess of alcohol in which too many of our grown people indulge. And perhaps I may be pardoned if I add that persons who are advised to restrict their diet in order that they may get rid of ailments from which they may have long suffered, either mild and long-continued ailments, or recurring attacks of a more severe or acute character, not infrequently suffer severely at first, and even for a long time after commencing a better way of living. Their bodies at once begin to eliminate the unassimilated stuff that is in them, a process which is as trying in its way, and brought about quite in the same way as the distress of the alcoholic who becomes a total abstainer. The

over-eater who adopts restriction (a class to which I fear most of us belong, though we do not like to admit it), finds himself greatly distressed by his new mode of life ; but how very unwise it is of him or her, if, as so many unfortunately do, he makes up his mind that this plan will not suit him, and if, because, for example, he does not sleep for a night or two, or otherwise feels a disagreeable aching void, he throws the whole thing up, and returns to his three-hourly meals, and to heavy and late suppers. We see the folly of this clearly enough in the case of the alcoholic, but the folly of the other person, though quite as great, does not, unfortunately, strike us so strongly.

CHAPTER V.

Some observations on the Circulation of the Blood and of the Lymph, as bearing particularly on the question of Food Supply to the body.

THE circulation of the blood, although so much has been heard of it since it was discovered by Harvey, the physician to King Charles the First (1578-1657), does not appear to me to have received the amount of attention, or, perhaps I should rather say, the sort of attention which it deserves. One's first impulse, no doubt, is to say, offhand, that it has revolutionised the practice of medicine and surgery ; but I do not know that this statement can be truthfully made. Some mechanical improvements have no doubt been introduced into surgery, as, *e.g.*, the tying of arteries in various places to cure diseases in the course of these vessels, or the removal or obliteration of veins for diseases of their coats, since the general course of the circulation of the blood has become known ; but the general practice of medicine has scarcely been altered at all, and any changes in it which have been effected have been introduced through considerations independent of our views upon the circulation of

the blood and lymph. It may help us to realise this better if we reflect that hundreds of years before the circulation of the blood and lymph became recognised as an established fact (yes, hundreds of years before — I had almost said thousands of years before—1400 years, at least), physicians were in the habit of feeling the pulse when they visited their patients. They thought, no doubt, that they were feeling the vein and not the artery, which last they believed to contain air and not blood (although in one passage Galen, who died A.D. 200, seems to say that the artery contained blood and not air). The very name artery means air-container (*aer* = air), and the ancients believed, so far as their somewhat vague notions can be made out, that the arteries were somehow the finer divisions of the wind-pipe, which they knew to contain air, and which they called *arteria aspera,* or the rough, hard air-container, so contra-distinguishing it from the ordinary or softer vessels, which they named veins. This was an easy mistake to make, or, at least, not so unlikely as we might have supposed, because, if we trace the wind-pipe into the lungs, we find it breaking up into branches which we now call *bronchi*, and these again into finer and finer ones, which we call *bronchia* ; and we also find the same division and breaking up characterise the behaviour of the pulmonary artery as it finds its way into the lung-tissue, the finest branches of it accompanying and being somewhat easily mistaken for the finest divisions of the

trachea, bronchi, and bronchia. But although the ancient physicians of Greece and Rome could not be said to know much about the circulation of the blood, that ignorance cannot be said to have militated much against their success as practitioners in the treatment of the sick. If we were to ask, indeed, in what respects they were worse practitioners than we are to-day, it would be difficult to reply satisfactorily. They had, or, at least, the best of them had, a general grasp of their subject, of its theory, and of the consequent art of medicine, which, with all our science, it were much to be wished many of us possessed now. The best of them knew, for instance, and were greatly impressed by the fact, that the body was one organic whole, and that, therefore, its diseases were one; that they were departures from normal conditions; that, therefore, it was more or less accidental whether this, that or the other member or part of the body suffered, since its sufferings were almost always local marks of general disorder, or, as we might say now (and as in fact some do say), most local diseases are the local expression of general states; and that, for their successful treatment, therefore, it is not and cannot be enough to confine our attention and efforts to the organ or part affected, but that if any real or permanent benefit is to be achieved, we must treat the organism as a whole. We may note in this connection what Cœlius Aurelianus, writing in the earlier part of the Christian era, said, in discussing what part of the body was at fault when

phrenitis, as he called it, was present. After saying that some said it was the head, others that it was the brain, and others that it was the membranes of the brain, he delivers himself of this reflection, profoundly philosophical as it seems to me, and exemplifying a point of view and a grasp too seldom attained to-day by our too highly scientific practitioners: *Nos igitur communiter totum corpus pati accipimus . . . . . sedplus pati dicimus caput.* "I therefore am of opinion that all the parts of the body suffer in common, but that the head suffers most of all." As if he had said, what is profoundly true: "Most local ailments are only local expressions of general states." The specialist is by implication here relegated to his proper place, and is informed, if he has wit enough to read the lesson presented to him, that it is not sufficient to remove an ovarian tumour, *e.g.*, and that if nothing is said at the same time or subsequently as to the causes which induced it, a positive damage may be done to the woman, who may, therefore, while considering herself cured, proceed to manufacture one on the other side, or may find herself in a few years suffering from cancer in the stump of the previous one. Or the child who has tonsils removed, and adenoids cleared away, may and certainly will subsequently suffer from colds, bronchitis, broncho-pneumonia, and the like, and by and bye probably from rheumatism or rheumatic fever, &c., unless at the same time or subsequently to the operation, his mother is advised to treat him differently from the way in which she

treated him before. For, if she does not, a worse thing may happen to him in the future, and so the operation which was intended to benefit may eventuate in damage and not in good. Evidently the same causes which enlarged the tonsils and caused the adenoid growths on the soft palate and nose will, if they are allowed to go on, tend to make the child ill again either in the same or in some other way. Or the middle-aged woman, who has a chronic discharge from her nose, may get it stopped, indeed, by having her nose cauterised by a platinum wire made white hot by the electric current, only to find herself in a few months suffering from cancer of the breast, which, being in turn removed, eventuates in cancer of the liver, for which there is no relief. These illustrations are, I may say, by no means imaginary, but are drawn from experience of cases in practice.

To return now to the question of the circulation of the blood, and the influence which its discovery has had on practice, let us listen to this statement of Celsus, the great Latin physician, who wrote and practised in the second century of the Christian era. In his *De medicina* he makes the following interesting observations. "On the other hand, very often the sun, a bath, exercise, fear, anger, or any other emotion of the mind, accelerates the pulsations (*concitat venas*), so that when a physician first comes in, the solicitude of the patient, doubtful and apprehensive respecting what opinion the physician may have of his case, is

of itself sufficient cause to excite the pulse. For this reason it is the part of a skilful physician, not to seize the patient's arm immediately as he enters the chamber, but let him sit down first, with a cheerful countenance, and enquire how he finds himself, and if he seems alarmed, soothe him with some plausible observations; then he may apply his hand to the body" (feel the pulse). "But how easily might a thousand other incidents excite the veins" (heart's action and pulse) "as well as the sight of a physician!" (Celsus III., 6).

This passage is very interesting, and has long been held to be so. It shews us how practically useful a physician might be, even although he did not know the course of the circulation of the blood, and had no proper appreciation of its facts and of its meanings; and it brings conspicuously before us the fact that the ancient physician, hundreds of years before the discovery of the circulation of blood, still felt his patient's pulse in order to judge of the condition he was in, and knew, further, that the state of the pulse might be affected by many things and circumstances, for which, therefore, due allowance had to be made, if he wished to properly appreciate his patient's state. Galen's views on the pulse are also well known.

The mediæval physician, as well as the ancient one, also felt the patient's pulse, but on the whole a more metaphysical attitude of mind characterised him than that found in his more ancient predecessor. The reader may perhaps pardon a

short digression, although I have no wish to make this a historical essay on medicine. But by introducing a few words regarding Dante's physiological ideas, we can, I think, perceive both the strength and the weakness of the mediæval mind. In the *Vita nuova* the following interesting passage occurs. When at about the end of his ninth year Dante first saw his Beatrice, at about the beginning of her ninth year, after describing her dress, which was of a most noble colour, a subdued and goodly crimson, he goes on to say—and this shews us by implication and also in fact, the physiology of the fourteenth century of the Christian era—" At that moment, I say most truly, that the spirit of life, which hath its dwelling in the secretest chamber of the heart, began to tremble so violently, the least pulses of my body shook therewith ; and in trembling it said these words : *Ecce deus fortior me qui veniens dominabitur mihi.*[1] At that moment the animate spirit, which dwelleth in the lofty chamber whither all the senses carry their perceptions, was filled with wonder, and, speaking more especially unto the spirits of the eyes, said these words : *Apparuit jam beatitudo vestra.*[2] At that moment the natural spirit, which dwelleth there where our nourishment is administered, began to weep, and, in weeping, said these words : *Heu miser! quia frequenter impeditus ero deinceps.*"[3]

1. Here is a deity stronger than I ; who, coming, shall rule over me.
2. Your beatitude hath now been made manifest unto you.
3. Woe is me ! for that often I shall be disturbed from this time forth.

Now these three spirits, the spirit of life associated with the circulation; the spirit of the anima, or the animate spirit, associated with the brain and nervous system; and the natural (or regetative or animal) spirit, associated with the digestive system—these three spirits formed one threefold chain among many threefold chains to be found in Dante's poems; and what perhaps strikes us most forcibly about them to-day is that they were metaphysical or philosophical in character, rather than scientific, although in this last connection each different spirit was associated with a different physical part and structure in the body. And we are also, I think, struck by the reflection that there seems to be an unnecessary number or multiplication of them. But let not the modern thinker too hastily imagine that he has advanced so very far in comparison with Dante's conception; for although to a great extent he has chosen to give these mediæval notions the go-bye, or to ignore them, he has not got rid of, and he cannot get rid of the fundamental conceptions underlying them, seeing that he, too, speaks of life, and vitality, and vital principle, and digestive power, and of mind and soul. Without espousing any view, or doing more than indicating the one which appears to me to be the most reasonable and the most comprehensive, I should just like to set down here once for all, and shortly, the three or four lines of thought which the human mind tends to pursue in dealing with structure and function. Plainly they

vary together. When structure is sound, function is healthy, and when unsound, unhealthy. Hence the view generally adopted by modern science that structure determines function, that, for example, the structure of the liver determines the secretion of bile. From this to the view that environment determines organisation, and even that matter determines mind, that the thing determines the thought, there is no long step. But, obviously, there is no more warrant for this view than there is for the other one that function determines structure, that organisation determines environment, that mind determines matter, and that the thought determines the thing. This is the second view, held by some philosophers, who find themselves in disagreement, not, indeed, with the material results of science, but with her logic. Obviously, as function and structure vary simultaneously, and co-ordinately, it does not matter, as far as physical results are concerned, which view we adopt. Then there is a third view, that the thought is the thing. And indeed there is more to be said for this view than perhaps appears at first sight. For if it is impossible for us to know what a thing may be *in itself,* so to say, and out of all relations to other things and to other intelligences, *to us* our thought of a thing *is* the thing itself. What we think it to be, that it is to us for the moment. The first explanation of things is material; the second is idealistic; the third is pantheistic, or may be thought to lead to pantheism. And there is a

fourth possibility which is this: that thought and thing form at each moment a new combination and a new compound creature, which begins anew its investigation of outside nature, so that we are always *becoming*, but *never are.* The problem of creation always has transcended and apparently always will transcend the wit of man; and we are face to face with it here in the inquiry whether function or structure was first. Who shall say whether day or night was first, or light or darkness? But it is not nececssary for us to solve the question, since day always succeeds night, and night, day. So structure and function move and change simultaneously and concomitantly. I have just said that whichever view we adopt, the facts remain the same. If we accept the fourth explanation of organic phenomena, or even the third, as the most likely and reasonable, I am not quite sure that this statement is true; but as the question has now become philosophical or theological, it would be obviously improper to pursue it further in a disquisition on medicine. Changes in function and structure, however, may, it is evident, be concomitant or successive effects of a common cause (like the alternations of day and night), as well as cause and effect of one another. All I want to show is that the questions raised by the mediæval mind, as by the ancient mind, are still with us, and that the calm assumption of modern science, that structure determines function, that matter determines mind, that the thing is before

the thought, may be utterly denied by an enquirer into the course of nature, who is as humble, as patient and painstaking, as unbiassed, as unprejudiced, as free from the influence of the personal equation as it is possible for mortal man to be. These considerations will again come before us in examining food supply, as they have been already raised by the causation of fevers, and the inquiring reader may find himself forced to raise them also on other occasions. In fact, whether soluble or not, these questions are continually arising, both in medicine and in life, and seem to exercise a fascination on the human mind.

The practical bearing, however, of these considerations on the circulation of the blood and lymph is considerable. As the blood goes everywhere in the body, it is obviously a matter more or less of accident which part of the body is affected. If the blood is sound, the parts of the body will be sound also, that is, in the absence of some more or less accidental cause, which may have for the moment disturbed any given part. But in this case the local irritation will soon be remedied by the powers of nature, by the circulation in the part of sound and healthy blood, and very soon all traces of the irritation will be gone; or at least the irritation will be in proportion to the magnitude of the exciting cause, the resistance of the part being great because of the soundness and wholesomeness of the blood, and the suffering, therefore, caused by the irritant comparatively slight. But let the blood

be unsound, loaded, let us say, with effete products —it is manifest that in this case the effects of the same irritant as before may and will be much greater; and the part affected, as also the whole organism, will be likely, nay, certain, to suffer much more than if the blood had been healthy. This would be seen in the case of a wound of the arm or leg, for instance, the suffering being proportionate to the magnitude of the injury and inversely as the healthiness of the blood, or proportionately to its unhealthiness. If, now, a person is exposed to a draft or cold or wind or damp, the same thing will be likely to happen. His weakest part will be affected, his lung or liver or stomach or eye, as it may chance; but the suffering will as before be proportionate to the magnitude of the exposure and to the unhealthiness of the blood (or inversely as its soundness or healthiness), all of which considerations shew that local affections or local inflammations are far more serious as being the marks of the general state of the blood, than they are as being affections of this or that or the other part. In fact, it is an accident whether the man exposed to the exciting cause, suffers in one part of his body or in another, and the same general lines of treatment which are proper and suitable for the one will be proper and suitable for the other. Also, if two organs are affected, say, *e.g.*, an internal organ and the muscles of the back, it is evidently much more likely that both the affections are caused chiefly by the state

of the blood that goes to both, than that the internal inflammation causes the pain in the back-muscles, or than that the affection of the back-muscles causes the inflammation of the internal organ. The bearing of these considerations on practice, simple though they are, is considerable. Sometimes, too often indeed, we are advised that treatment, say, of an internal organ will cure the muscles of the back, whereas it would be quite as rational to expect to cure an internal inflammation by treating the back-muscles (driving needles into them, for example). But by treating the blood, by altering its condition, it will be rational to expect to cure both. In fact, both sets of effects are concomitant or successive effects of a common cause, and not cause and effect of one another. Further, also, a pair of "specialists" are not required for the management of that patient. He or she is, in fact, better without them, as, if they were narrow-minded specialists, the treatment of the one would conflict with the suggestions of the other; while if they were broad-minded and judicial, they would both advise the same general line of treatment. The view, however, which we take of the condition of the patient, and, consequently, the treatment we recommend for him is precisely analagous to the third or fourth view mentioned above respecting the relation of structure to function, and may, I hope, justify to the reader my introduction of these apparently outside and irrelevant considerations.

Besides feeling the pulse, I may also remind the reader that, centuries before the discovery of the circulation of the blood (and lymph) it was customary among doctors to resort to blood-letting, and to leeching, as well as to blistering, for the relief and cure of diseases—and yet one would have thought, *a priori*, that at least the two former of these methods depended directly upon the discovery of the circulation of the blood, and would appear useless and inept without it. The great reason, indeed, why blood-letting has been to so large an extent given up is not, in my opinion, because the discovery of the circulation militated against it—on the contrary, it justified and encouraged it, if anything—but because it came to be recognised that removal of a quantity, say a pint, of blood, while it might to that extent remove some of the *materies morbi* of disease, could have no influence on the rest of the blood in the body, and could, therefore, do nothing towards removing the *materies morbi* or *materies morborum* from the remaining 10, or 11, or 12, or 13 pints of blood in the body. At least this is the consideration which ought to have determined the cessation of blood-letting. But, in point of fact, it was not even this which did so determine it, but a feeling that men's bodies were weakened by the process. We are too apt, it seems to me, in considering the present state of knowledge, and comparing it with that of past generations, to imagine that we know a great deal more than our predecessors. No doubt there are

whole domains of knowledge within the power, more or less, of men of the present day, which were not even dreamed of in past times. Chemistry, biology, physiology and pathology, *e.g.*, did not exist in their present form in the times of Hippocrates, or Celsus, or Galen. And yet, as to conduct, or as to the medical management of illness, it is doubtful whether we are much in advance of our predecessors. We are at least, I feel sure, not so much in advance of them as we so often and so complacently suppose. The role of the progress of science is, after all, not so much, or at least not so much as we suppose, to point out new methods of inquiry into the constitution and course of nature, as it is to corroborate, by the use of newer and more accurate methods, what we may see and perceive by means of the proper use of the senses. I have already mentioned the feeling of the pulse and the instruction obtained from it by men who knew nothing of the circulation of the blood and lymph, and I will now mention one or two other ideas known to the ancient physicians, so that we may be able the better to gauge the truth of what I am saying. The most ancient observation, perhaps, in medicine, was to the effect that we cannot feed the sick as we feed the well; and Hippocrates, writing, say 450 B.C., mentions this as an observation of the physicians who were "ancient" in his day, that is, as an observation made by men living 1000 or 1500 years B.C. If this be considered an observation, so trite that

no one could fail to notice it, what shall we say to the observation of Hippocrates himself, already mentioned on page 117, that less food is required in hot weather than in cold? This seems to me a remarkable observation to be made by a man who lived many centuries before chemistry or combustion, or the relations between heat and oxidation, or between heat and physiological or physical energy had been dreamed of. But he saw, and correctly saw, by observation, what was only demonstrated, or let us say, what had new experimental light thrown on it many generations later. The last point I will mention at present is perhaps the most important of all, and it is an observation of Hippocrates to the effect that people ought to be fed in proportion and in relation to the work they have to do. We must inquire, he says, "whether the food has been too much for the work to be done, or the work too great for the food, or whether they have been proportionate one to the other. For according as one exceeds the other, *diseases set in,* while from their *equality with one another, health arises.*" Although there has been so much "advance" in chemistry, physics, biology, physiology, and pathology, I think my friend Dr. Dewey is justified in his question, whether the sons of medicine have ever given to the world physiology of equal or greater importance in these nineteen hundred years since Christ? And I repeat that these three observations (with others I could mention), tend to

induce and corroborate the view that as regards the conduct of medicine, the progress of science goes to strengthen us in forming conclusions, which, by the unaided but candid use of the senses, we could observe and know without her.

As bearing on the circulation of the blood and lymph and its relation to food-supply, certain fundamental considerations arise to the inquirer. As a circle has neither beginning nor end, or as it may be viewed, if we are so minded, as all beginning or all end ; so in studying a round or circle of organic processes, all mutually connected with one another, it is immaterial at what process or point in the circle we commence our study—we shall return to that point or process again. Nevertheless, there are some ways, and some points of view, from which more instruction will be obtained from the study of the mechanism than if it is approached in other ways. In the case of a steam engine driving looms or wool-combing machines, it is immaterial whether we commence our study of its action at the shaft, which, by its attachment to the crank, turns gearing which turns leather bands, causing the back and forward movement of the looms and the to-and-fro movement of the shuttles, or whether we commence our study at some other point. Through bands and driving wheel and eccentric, through piston and cylinder and boiler and the fire, which is the source of the motion of the machine and of the mill, we come back to the point from which we started. But it is more illuminating and

instructive to begin with the fire, and proceed to examine boiler, cylinder, piston, &c., than in other ways; or even to commence with the pump which sends the water into the boiler.

The analagous method by which the organic processes of the body are to be examined in order that the animal machine may be understood, and the best work provided by it, is through a study of the digestive processes; and, therefore, a very short account of these processes must be inserted here, in order to draw attention to them a little more at length, and so that we may be better able to understand and appreciate the reasons for the advice to be offered regarding the times and quantities of meals. This account, though popular, must be correct, so far as it goes, but will not, of course, be so elaborate as if it formed part of a technical course of physiology. At the same time, it should be sufficient to enable us to understand the round or circle of the bodily mechanism. First, then, the food, being taken into the mouth, undergoes the action of mastication by the teeth, whose function is mainly one of mechanical trituration. At the same time (and the better will this be effected, the more completely is that mastication or trituration and grinding by the teeth performed), the food ought to be slowly and well mixed with saliva in the mouth, coming from the salivary glands. Foods have been divided, since the time of Liebig, the great physiological chemist, who died in 1872, into four great classes, according to their chemical

composition. (1) Those foods which contain nitrogen, and are called proteids or nitrogenous foods. The most important examples of this class are such foods as meat and flesh of all sorts, fish, eggs, milk, cheese, the cereal grains, and pulses belonging to the pea and bean tribe. (2) The group of foods represented by sugar, arrowroot, sago, tapioca, &c., and called non-nitrogenous or carboniferous or carbonaceous. The latter names are, however, bad, since the nitrogenous foods also contain a carboniferous part. With the division, however, of foods into nitrogenous and non-nitrogenous, no fault can be found, although the notions of Liebig have been considerably modified by subsequent inquiries as to the parts played by the different kinds of food in the economy. The non-nitrogenous foods have been divided into two groups, first, the sugars and starches already referred to, and secondly, the fats (oil, butter, cream and fat), which form the third division of food stuffs. The first division of foods, the nitrogenous, are alone capable of repairing completely the waste of the body-flesh or muscle, since it contains nitrogen in its composition. But although the carboniferous or carbonaceous foods contain no nitrogen, the nitrogenous foods contain considerable quantities of carboniferous stuff, and are therefore capable of performing all the functions in the body which can be performed by the non-nitrogenous foods, with the power of building up again the nitrogen-containing tissues in addition. The great function which the non-nitrogenous

foods perform in the body is that of maintaining the body-heat, and this they do both by the sugar and starch group and by the fats, but the latter, as we see from the use made of them by denizens of very cold regions like the Arctic and Antarctic regions, have greater power in maintaining bodily heat than sugars and starches. The fourth division of food-stuffs is the mineral portion made of salts of alkalies for the most part, as compounds of potash, soda, lime, and common salt, with smaller quantities of sulphur, &c., entering into their composition. Lastly, and so important is it that we may consider it as forming a fifth division of food stuffs, water enters largely into the composition, and forms from two-thirds to three-fourths both of the foods used in the body and of the body itself.

Now the saliva coming from the salivary glands in the mouth, and being there well mixed with the food, has little or no action on the proteid or nitrogenous part of the food proper, has little or no action, *e.g.*, on fish, flesh, fowl, or eggs or cheese; but exerts, on the other hand, a very considerable effect on the non-nitrogenous starches and also on the non-nitrogenous portion of nitrogenous foods—of bread, for instance, or of rice—the starch of which it converts into grape sugar. Starch must be converted into grape sugar before it can be used in the economy, so that the need for good chewing and for efficient mixing with saliva becomes more apparent from this consideration. The food, being

now ready for swallowing, is passed along the gullet (which exerts comparatively little action on it) into the stomach. There it is met by the gastric juice, considerable quantities of which (from a pint to two pints) are secreted into the stomach from its walls, each time that food is taken. The gastric juice is sometimes called the gastric acid because it has an acid reaction, while the reaction of the saliva is alkaline. A proper appreciation of the fact that so much gastric juice as two pints is poured out when a meal is taken, is highly important in determining the quantity of food that ought to be taken at a meal. The children of a past generation —the advice is not so common now as it was then, nor so common as it ought to be—were advised to stop eating before they felt that they had had as much as they could take, or before, as the phrase was, they felt full. Now this was very wise advice, for if we go on, either children or adults, eating till our stomachs are full, how are we to make room for the pint or the quart of extra material when gastric juice to that amount has been poured into the stomach? But we *must* make room for this extra quantity of material, because the presence of gastric juice is absolutely necessary if food is to undergo gastric digestion and to be converted into chyme. There can be no doubt, indeed, that the too widespread habit of eating to satiety, even if it is not carried quite to fulness, is, after the necessary amount of gastric acid has been added to the stomach contents, the chief cause of that feeling of

distress and weight or pressure which is so apt to be felt by all of us from half an hour to two or three hours after eating a hearty meal. Taking a less amount of food would have allowed more room for the necessary gastric acid, and would have prevented the feeling of weight and heaviness. The fact also incidentally alluded to that digestion in the stomach goes on for hours, shews, by the way, how futile is the advice sometimes given to us that we should lie down for say half an hour after taking a meal. In half an hour, or even an hour, the digestion of a townsman or townswoman has reached only its initial stages, and if it is necessary to lie down half an hour or an hour after eating, it is because we have taken more at the meal than was good for us. To suggest that we should lie down for three or four hours after eating would probably meet the case; but to do this would be to behave like a boa-constrictor, and would be to raise eating into the "chief labour of life," while no amount of physiological rest after food consumption could ever undo the evil effects of over-ingestion ot food, especially if that process were frequently repeated.

Another practical conclusion of the utmost consequence, besides the one just come to, that we ought to stop eating before satiety is reached, is that we ought to eat slowly. This is so obvious that it ought almost to be unnecessary to state it; for if saliva is to act on the starchy portions of our food, it must be well mixed with the food in order

to do so, and this can be effected only by thoroughly chewing the food as it is taken. Very many persons, perhaps even a majority of us, make the great mistake of eating too quickly, and so of failing to masticate and triturate properly the food we take. Eating slowly is in fact a help against the error of eating to satiety, because we have more time to appreciate the state of fulness that the stomach gets into after ordinary eating, and so we are helped by slow eating to stop before we have taken too much. The simple device of chewing each morsel of food taken, say twenty or thirty times (some have said even forty times), before swallowing it, would greatly help towards forming the habit of slow eating; and this in turn, as it would greatly aid digestion and assimilation, blood-making, and tissue nutrition, would have the most potent influence on the maintenance of health and the prevention of illness.

The gastric juice or gastric acid acts specially on such nitrogenous foods as meat, fowls, fish and cheese, and, helped of course by the preceding action of the saliva, and by the rolling action exerted by the muscular walls of the stomach itself, converts the whole contents of the meal into a grey grumous fluid mass called chyme. Not yet, however, is the food ready for absorption. It has to be passed out of the stomach into the small intestine; and immediately on its reaching this part of the digestive tract, it meets with the secretion from the pancreas and that from the liver.

These secretions have again an alkaline reaction. The former finishes the digestion of any starch which may have escaped the action of the saliva in the mouth, converting it into a state fit for absorption, either directly into the blood, or indirectly to the same destination by means of the intestinal villi. These last structures are found in the small intestine, and contain each a small lacteal vessel, whose mouth opens and absorbs some particles of chyle. These particles are passed on by the lacteal vessels, and find their way into the thoracic duct, in the way to be immediately described. The action or influence of the bile from the liver on the other hand is mainly to prevent putrefaction in the highly unstable contents of the digestive tract, and to complete the digestion of particles of fat, butter, cream, &c., and fit them for being mixed with the contents of the small intestines, the chyle. This chyle is completely elaborated and rendered ready for absorption on meeting with the alkaline secretion from the small intestine itself. Entering the venous blood from the small intestines, it is carried by the portal vein to the liver, while some of the chyle-particles enter the lacteals and thoracic duct, being emptied by it directly into the venous blood at the root of the left side of the neck. This venous blood, if we follow it in its course, will be found, by means of the superior or descending vena cava, to be carried to the right side of the heart. How much of the chyle finds its way by means of the portal vein to the liver, and how

much by means of the lacteals and thoracic duct to the superior vena cava, does not seem to be quite known. In any case, that which goes to the liver after being laid out there by means of its very extensive circulation, and after having some very important changes effected in it, finds its way back again by the hepatic vein into the general venous circulation, so that in time all the chyle is emptied into the venous blood, either directly by means of the thoracic duct, or indirectly through the portal circulation. Being carried in these ways to the right side of the heart, the blood, dark and venous, and loaded with the carbonic acid gas and other products of oxidation, which it has received from the tissues as it was passing through them, is carried by the pulmonary artery to the lungs right and left. There it gives up its carbonic acid gas or carbon-dioxide, and takes in oxygen from the inspired air, changing coincidently its colour from crimson-black to scarlet. There ought to be no oxidation or combustion in the lungs according to the laboratory physiologists, a mere passing of carbon-dioxide from the venous blood taking place, accompanied by the entrance of oxygen from the air. But when, from the blood containing too much material in it from unused food, an exudation from the blood has taken place into the mucous membrane lining the lungs, or into the substance of the lungs itself, it may happen that oxidation and excess of combustion does occur here as in any other inflamed place in the body. When

this is so, no doubt the limits of health have been transcended, and disease has set in. Physiology has begun to shade off into pathology. But this is unfortunately far too commonly the case, pulmonary affections or diseases of the respiration being among the very commonest affections of the body. The figures in Chapters II. to IV., regarding the incidence of disease, show how common they are.

By the pulmonary veins, which therefore contain arterial blood, the oxygenated and depurated blood is carried back from the lungs to the left side of the heart, thence to be distributed all over the body for its nourishment, for its waste-repair, and for building up its strength. We see from this short summary how the function of food is to make blood and to enrich the blood, and how it is the function of blood to nourish the body. It is only therefore by ellipsis that we can speak of the function of food as being to nourish the body, to repair its waste, and to build up its tissue. The food does all these things, but not directly or immediately, since it does them mediately or indirectly through the blood. And in the attempt to understand the diseases and ailments of the body it is absolutely necessary that we should keep these steps of the processes of nutrition and of digestion separate in our minds. By means of this consideration also we can introduce order into the study of medicine and of diseases, and so substitute simplicity for chaos and confusion. The multiplicity of diseases, for

instance, the immense number of them which come under our notice, either because we ourselves or our friends or acquaintances are suffering from them, are apt to fill us with despair as to the possibility of our ever being able to understand them or see their causal connection. But let us once get into our minds the facts that the blood goes everywhere all over the body, and that the blood is made by changes which through the digestive processes are effected in the food, and then how simple do these apparently complicated diseases become. For if the blood goes everywhere all over the body, then it may nourish the various parts properly, if it is itself in good condition, that is, if digestion or assimilation has been properly effected, and if the proper quantities and qualities of material have been poured into it; while, on the other hand, it may fail, and will fail to nourish the various parts properly, if it is itself not in good condition, that is, if digestion or assimilation has not been properly effected. In the former case the various tissues will be so properly nourished that they will be healthy; and so, as they are acting normally and painlessly in reference to their various functions in the body, and in relation to one another, we remain unconscious of their very existence, feeling only the general pleasurable sensation of health and well-being. But if, on the other hand, the blood going to the various bodily tissues has not been properly made by the digestive processes; if, for example, as often happens, it is

loaded with badly assimilated materials, we can easily understand how it may deposit in any of the various tissues some of its ill-formed material, and how this, by disturbing the exercise of the various functions and the relations of these functions to one another, may translate itself into pain and discomfort, and may give us an unpleasant consciousness of the existence of parts and organs, which knowledge we might otherwise have been enabled to escape. In this way we may suffer from bronchitis, or pneumonia, or pleuritis (pleurisy), according as the ill-made and waste-laden blood deposits an exudation into the mucous membrane lining the lungs, into the lung tissue itself, or into the pleura or serous membrane covering the lungs. And in the same way any and every organ and tissue of the body may be affected, the names of the affections of different parts being as various as the different tissues and organs themselves, while their various symptoms differ from one another according to the very various functions of the different parts affected.

I have already referred shortly to the portal circulation, and with one or two words more our sketch of the blood-making processes may be completed. The blood-vessels which come from the stomach (where probably some absorption of chyme takes place, although the mass of it passes on through the duodenum into the small intestine), those blood-vessels which come from the small intestine, filled with the products of digestion

there, that is, with the chyle ; as well as the blood-vessels which come from the spleen and pancreas, join together to form the portal vein, as it is called, because it is found at the gate-way or door-way (*porta* = gate or door) so to term it, of the liver. The portal vein carries its contents received from these four most important viscera, the stomach, the small intestine, the spleen and the pancreas, to the liver, which is the largest digestive viscus and one of the most important organs of the body. This organ, which weighs about four pounds when out of the body, is said by physiologists to contain, on occasions, about 29 or 30 per cent. of the blood contained in the body of a rabbit. By generalisation, this proportion is supposed to obtain in the case of other mammals and man. Whether it does so in this proportion or not does not really very much matter. What does matter is that the liver receives through the portal circulation an immense quantity of blood. It is indeed difficult to believe that in man the quantity of blood received by the liver can reach so high a proportion as 30 per cent. at any one time, since this would imply that three or four pints out of the twelve to fourteen pints of blood contained in the body would be in the liver at one time. As, out of the body, the liver weighs about four pounds, the addition of three and a half or four pints of blood would increase its weight to about double ; and although no doubt the liver differs from itself immensely in weight, according to the

different states of digestion, it is not easy to believe that from time to time it actually doubles in weight. That it varies very much indeed at different times is no doubt true, and is all that need concern us. And whoever properly appreciates this fact will be able to rate at its proper value the statement, for example, of a patient, who said that his doctor had been able to reduce the width of his liver by about an inch since beginning his attendance on him. If he had told us the comparative times of digestion at which the measurements were made, we should have been better able to judge the value of the statement; but that an empty or comparatively empty liver should be reduced by an inch when it had been overgrown or hypertrophied by that amount, is a statement which would require to be supported by a good deal of evidence before it was accepted.

While I am on this subject let me mention some other statements of the physiologists regarding the facts of the distribution of the blood in the body. These facts, let me repeat, are taken from the rabbit, and what amount of weight ought to be attached to them when applied to the circulation in man must be carefully considered. The muscles, they say, contain about 30 per cent. of the blood also. If this is true for man, then we should have about four pints of our blood in the muscles. I think we have quite that proportion. On the enveloping membranes of the muscles, blood-vessels are carried freely, as also on the

finer forms of envelopes which surround bundles of muscular fibres, down through still finer and finer processes, till we finally reach the finest coverings of all, or the sarco-lemma, as it is called, enveloping the ultimate muscle-elements. On all of these, arteries finer and smaller according to the increasing refinement of the enveloping membranes, are carried till they are lost in the very finest sarco-lemmatous processes, where, however, lymph-spaces are not absent. It can therefore readily be believed that four pints of blood or more are distributed to the muscles of the human body. And the significance of this seems to be very great. The muscles are the active agents of the body, or the agents through which the active powers of motion, locomotion, and work are performed. It is likely, therefore, that they will require a large supply of blood, in order that their active work may be possible; and whether this supply amounts to 30 per cent. of the whole of the blood or not, it is evident that the supply of blood to the muscles is free and abundant.

Continuing our examination of the blood distribution in such an animal as the rabbit, we come upon other very interesting facts. The heart and great blood vessels, the great arteries and veins of the body, with the addition of the lungs, do not, say the physiologists, contain more than 22 per cent. of the total volume of the blood. This is very curious when we reflect that we are in the habit of thinking of the heart and vessels as the

organs of the circulation *par excellence.* If this proportion holds in the case of man, only about three pints of blood out of his fourteen, or only 2·6 pints out of a total of twelve pints, will be contained in the heart, blood vessels and lungs at any given time. That is to say, the blood-contents of the organs of circulation and of respiration added together do not reach the amount contained either in the liver or in the muscles (assuming, of course, what is not certain, that what is true in the rabbit obtains also in the case of man).

The physiological significance of these facts must be very great, and we may reasonably draw these inferences .—(1) The liver exerts an enormous influence on the blood-making processes ; (2) The muscles play a great part in the blood-using processes. In order that they may do the work of the body they require a large blood supply, which, if they are active, they call upon the food to supply. And, no doubt, the greater the muscular activity of the body, the larger will be the amount of blood in the first place, and of food in the second, which they will require. On the other hand, the less the bodily activity, and the lighter the muscular work done, the less blood do the muscles require, and, therefore, the less food. Now let us set beside these facts another one also vouched for by the physiologists. The brain, they say, of a rabbit, and its spinal cord together, the governing and controlling structures, ordering and willing so far as a rabbit can govern movements, can order and

can will, can think and can feel, the cerebro-spinal nervous system contains only about 1·5 per cent. of the blood within it at any given time. If this is true of man, his brain will contain about one-fifth of a pint of blood out of the twelve or fourteen pints contained in his whole body. It may be said that probably the brain of a man is better supplied with blood in proportion than is the brain of a rabbit ; and this does not seem to me an unreasonable idea, considering what very different work a man's brain performs from that done by a rabbit's. But if the human brain is twice as well supplied with blood in proportion as a rabbit's, this would only give two-fifths of a pint of blood in the human brain, as compared with three or four in the muscles ; while, if the human brain contains even 5 per cent. of the blood, as compared with 1·5 per cent. in the rabbit, this would only amount to from three-fifths to seven-tenths of a pint of blood. Even then, on a rather high and free computation, we seem to be driven to infer that the brain does not require a large amount of blood for the conduct of its operations, while the requirements of the muscles are very considerable, in order that they may perform their work, at the same time that the requirements of the liver for the purposes of digestion and assimilation of food are very great also. The supply of blood, therefore, seems to require to be large in order that the lower functions, so to call them, of the body, the functions of locomotion and of digestion, should be well per-

formed; but in order to the performance of the higher functions of cerebral activity, ordering, feeling, perceiving, judging, thinking, reflecting, and willing, not nearly so much blood seems to be required. This is a rather remarkable conclusion to come to, but I think it is a sound one ; and there are some very remarkable physiological facts which point in the same direction, and appear to bear it out. For instance, it is surely a very remarkable fact that although the blood supply all over the body, and in all its parts, except the brain, is and remains under the government and control of the heart, it is not so in the brain; but the blood supply of the brain passes under a different government whenever the internal carotid artery enters the lacerated opening in the temporal bone in order to pass in to supply the brain. Whereas in the common carotid artery in the neck, before the internal carotid artery branches off for the supply of the brain, and whereas in the external carotid artery going to the face and *even to the brain membranes*, and in all the other arteries of the body wherever they are distributed, the circulation is and remains synchronous with the pulsations of the heart, that is, heaving at the rate of from 60 to 90 times a minute, the motion of the blood in the vessels of the brain itself is synchronous not with the heart's pulsations, but with the respiration, *i.e.,* it heaves and throbs and moves only at the rate of from 13 to 17 or 18 times a minute. The full significance of this remarkable arrangement does

not seem to be appreciated, but it appears to me not unlikely that it is partly at least because the brain does not require a very large amount of blood supply in order that it may perform its functions well, and that, in fact, it is rather hampered, than otherwise, and hindered if a very large supply of this fluid is sent to it. Another fact seeming to point in the same direction, is that the arteries in the brain end as end arteries, each to its own small portion of cerebral substance, and do not anastomose with other fine arterial endings, going to neighbouring parts, as other arteries do in other parts of the body. The object of this may be to ensure that if by chance a fine cerebral vessel becomes blocked, the disease and the impairment of vitality and power consequent on this condition may be confined to its own portion of cerebral substance and be prevented from spreading to other portions of the brain. But the fact, so far as it goes, seems to have for its object, or, at least, for its effect, the limiting of blood supply to the brain. Now as blood is made from food, this seems to mean that persons who use their brain largely should not take much food lest they should make too much blood, which, finding its way in too great volume to the brain, might cloud and interfere with the finer and subtler working of that governing and controlling and thinking and feeling organ. This seems to be a remarkable vindication by physiology and anatomy, of the correctness of insight of the poet who sang that there was a close connection

between "plain living and high thinking." The finer functions of the brain, indeed, do not seem to be performed through the blood supply so much as secondarily they seem to be associated with the formation and with the state or quality of the fine lymph which is found in all the brain cavities or ventricles, as they are called. This fine, subtle, clear, spirituous fluid exists to a considerable extent, especially in the large lateral ventricles in the anterior parts of the brain, as well as in the smaller third ventricle, and has set apart for its production a special arrangement of blood vessels known as the choroid plexuses. (There is a choroid plexus even in the fourth ventricle.) These choroid plexuses are collections of blood vessels carried on prolongations of the finest brain membranes, and from them seems to be secreted the fine spirituous fluid, which used to be called by older writers animal spirits, and which they appear to have thought to be the medium through which the higher powers of the brain were conducted. As this fine fluid or animal spirit (the animal spirit or animal spirits we see to be fundamentally a physical fluid, although in mediæval and modern language the expression seems never to be used in this sense, but always in a metaphysical or psychical or spiritual sense) is found in all the cavities of the brain, as it flows between the membranes of the brain, moistening and lubricating them; and as it passes even between the membranes or sheaths of the nerves also, which pass out from the brain and spinal cord,

we may be quite certain that the function of a fluid so widely distributed must be an important one, and no doubt it plays a great and important part in acting as a medium to hold intact the nexus between the brain cells and the more or less remote parts of the body, which in so remarkable a way recognise the authority of the brain, and respond to its commands.

But it is now time to consider more thoroughly the nature, and character, and origin, and relations of this fine spirituous fluid having this peculiar and interesting relation to the brain and nervous system. It belongs no doubt to the same class of secretions as are found all over the body, under the name of lymph, and may be considered as a finer form of the same, distilled, so to say, or secreted from the choroid plexuses in the brain. How important the lymph is, finer and coarser, will be apparent when we reflect that, although the exact quantity of it in the body is not known, it amounts in the aggregate to probably not less than thirty or forty pints. It is in fact not unlikely that occasionally, or even frequently, more than this quantity is present in the body of an average sized man, who has, on the other hand, only twelve, or thirteen, or fourteen pints of blood. This fact of itself invests the lymph and its circulation with great importance. Let us try to realise more clearly what the lymph is. It is the watery or clear fluid basis of the blood, uncoloured, or very slightly coloured, and it contains also a considerable number of

corpuscles, some not larger than very small pin-point granular masses, and some a little larger, some not unlike in size to the corpuscles of the blood, though not coloured red like them. It is collected in interstices that exist between the layers of the tissues of muscle sheaths. There are spaces also in the very substance of connective tissue, like muscle sheaths, nerve sheaths, periosteum, sarco lemma, neuri lemma, &c., &c., and in these spaces, sometimes lined by very fine layers of membrane, called endothelium, the lymph, or watery part of the blood, collects. From these interstices, in the substance of the connective tissue, and from the spaces between its layers, the lymph is conveyed away by channels which, gradually becoming lined, form lymphatic ducts and lymphatic vessels, which conduct away the lymph collected in this way, and take it to structures, called lymphatic glands, of which more detailed mention must immediately be made. Meantime, we must bear in mind that the lymph is itself collected from the blood after the blood has been carried to the various structures of the body, in order that it may nourish them. The arteries which convey the blood to these various structures (the definition of an artery is that it is a vessel which conveys blood *from* the heart, while a vein is a vessel which conveys blood *to* it), break up into finer and finer branches, until they end in capillaries, wide enough only to convey one blood corpuscle at once. The blood corpuscles, conveyed in this way, find themselves in relation with the

ultimate cells of the tissues, being separated from them only by a layer of the finest membrane, through which nutritional changes pass between the blood corpuscles and the tissues, these changes having the effect of building up, repairing, strengthening and vivifying the tissues so as to fit each different form for the performance of its own special function. But after these nutritional changes have been effected, and after the blood has parted with its nutritive materials to the tissues, something is still left over, and this something is picked up in the form of lymph, as above described, and finds its way first into lymph spaces, and then into lymph ducts, to be carried further into the body, where we must now follow it. The lymph ducts go in the vertebrata (there is no lymph circulation in invertebrata, we are told—the true inwardness of which fact is of intense interest) to the lymphatic glands, which are found in numerous places in the body, particularly at the sides of the neck, in the arm pits, and in the groins, as well as in many internal parts, as the roots of the lungs, intestines, &c. Entering these lymphatic glands, they coil about in an intricate manner in their interior, and an important elaboration of the lymph must occur, because after it has passed through these glands, it is found to contain many more corpuscles, generally also larger corpuscles than it contained before it entered the glands. In some cases the lymph passes through as many as two or three of these glands before it joins the thoracic

duct. For it is important to bear in mind that the lymph is carried into the thoracic duct. Now as the thoracic duct contains the particles of chyle which have been absorbed by the villi and lacteals of the small intestines, and as the contents of the thoracic duct itself are poured into the large vein at the root of the left side of the neck, for the purpose of being mixed with the blood, in order to enrich the blood and enable it to perform its function of nourishing the tissues of the body, and repairing their waste, it is evident that the lymph also assists in these important processes. The lymphatic vessels, therefore, may be considered as existing for the purpose, or at least chiefly for the purpose of collecting, and so of getting used over again any constituents of the blood whose powers have not been wholly used up in the nutritive processes. The lymphatic vessels are a part literally of the *economy of nature*, which will not allow, so to say, if she can avoid it, any waste to take place in the body. Any nutritive materials, whose powers have not been completely exhausted, are re-collected by these vessels, and carried by them to join the thoracic duct charged with the products of digestion, so that, after being elaborated by the glands, they may be used again in the body without waste. The flat or expanded structures, in which the lymph spaces and the lymph vessels arise, form investments and supports for most of the organs of the body. They are called fibrous tissues, according as they form investments,

or sheaths, or supports for muscles, nerves, bones, and the outside of joints; or serous membranes, according as they form investments and supports for organs like the lungs, liver, intestines, heart, the inside of joints and of internal organs generally. Their structure, although seemingly made of one layer of fibrous or serous tissue, really consists of several or many layers, as can be easily seen when, through inflammation, or congestion, or engorgement with lymph, the layers have been somewhat separated from one another. In between these layers, and in interspaces which are scarcely visible in the substance of the individual layers themselves, the lymphatic system of vessels begin, and these interspaces become filled with the watery part of the blood, containing also white granular and corpuscular particles, after it has been squeezed out of the organs, and particularly when it has been squeezed out of the muscles, after the blood has parted with some of its nutrient materials for the life and energy-supply of those organs.

Now in order better to appreciate the part played by this arrangement, let us attempt to realise what the effect of too frequent feeding will and must be in the body. When this interesting arrangement has come into operation, some hours after food has been taken, and when the lymphatic vessels are engaged in picking up this watery and corpuscular part of the blood for re-use in the body, let us suppose that the person in question has meantime taken another

meal. There is the most unmistakable evidence present from time to time by which it can be demonstrated that, when persons are engaged in performing the light duties of town life, food has not left the stomach for five or six hours (frequently much more than five or six hours), after it has been taken. Even after it leaves the stomach, let us remember that some considerable time longer must elapse before, having reached the small intestine, it has been taken up into the blood in the form of chyle by the thoracic duct, or by the portal vein. Now let us suppose that in a case which requires five or six hours for gastric or stomach digestion to be completed, a new meal is taken in four hours after the previous one. Let us suppose that a person who breakfasts at 9 o'clock takes dinner or lunch at 12-30 ; or, since digestion is probably more rapid in the forenoon than it is later in the day, let us ask ourselves what will happen if a person who has had lunch at 1-0 or 1-30 takes afternoon tea, and cake, and bread and butter, at 4-30 or 5-0. Plainly before one meal has left the stomach (not to speak of the small intestine), another one is ingested some hours before any need for it can possibly have arisen in the economy. Evidently, therefore, before digestion of the previous meal has been completed, the digestion of the next will have to be begun, and more gastric juice will have to be poured out into the stomach, before the previous contents have been properly dealt with. In fact, digestion will be going on at two different stages in

the same stomach at the same time. Now if even it be admitted that this might conceivably happen once or twice without doing much damage (but even once or twice is once or twice too often), it is quite obvious that the repetition of the process must be most deleterious. Besides the fact that digestion is going on at two different stages in the same stomach at the same time, and that all the other digestive processes are apt to be simultaneously disarranged by the same or similar causes, we must consider the changes effected by the same causes in the lymph circulation. Obviously too much lymph must be finding its way into the blood through the thoracic duct. Before one supply has been properly elaborated and mixed with the blood current, another supply is on the way. This must interfere with proper assimilation in the first place, and in the next must lead to overloading of the blood with excess of nutritive material. Then, nextly, where such too rich blood has been carried to the muscles to keep up the heat of the body, and to enable them to perform mechanical work, there will be a larger over-plus in the form of lymph to be absorbed by the lymph spaces, and to be thence passed on to the lymphatic trunks, by them to be carried to the lymphatic glands for further elaboration, before the contents pass on to be poured through the thoracic duct back again into the venous blood. The lymphatic vessels, let us suppose, do this work well. They rid the blood of material which is unused, and pass

it on for re-use in the economy, But long before they have succeeded in doing this, another supply of lymph and chyle has entered the thoracic duct and the portal vein, and from these has been poured into the blood. The blood carries it all over the body, and particularly to the muscles, for conversion into heat and into mechanical work. The same round re-commences, the same overwork is thrown on to the lymph spaces, and the lymph vessels, and the lymphatic glands. The muscles become heavy and achy, and the person feels languid and unable or unwilling to move, and low and weak, not from taking too little food, but from taking it too often and too much. Nevertheless, she feels weak, and further finds herself relieved for a short time by taking more food, the heat of hot tea (which is accompanied by bread and butter or cakes) stimulating the digestion to assimilate some of the unused stuff in the digestive tract and in the blood, while unfortunately the efforts of the lymphatic system to relieve the blood are rendered nugatory by the perpetual re-ingestion continually going on, at too short intervals, of food in excess of the requirements of the body. Obviously the economical arrangements of nature may be opposed or thwarted by unphysiological management. Before the lymphatics have had time to collect the lymph, more materials for lymph formation have been ingested into the body. The new digestive processes occur too soon. Too much material finds its way into the blood, and, especially

if this arrangement is repeated frequently and at too short intervals, it is evident that the lymph spaces may become blocked, and that the lymphatic glands may become congested and inflamed. When the congestion and inflammation proceed further, pus-formation or suppuration occurs, and in this way we can easily account for those enlargements of the glands and for those disfiguring suppurations and abscesses in the neck which often occur among the people, and especially among the children, of this country. In the same way, and from the same causes, the lymph spaces in a serous membrane like the pleura or that lining the interior of such a joint as the knee, may become over-filled with lymph, and so an attack of pleurisy or pleuritis may ensue, or a white swelling, as it is called, may occur in the knee joint of a child. If these are theoretical ways in which such diseased conditions may arise, I have no doubt that they are also in most instances the practical ways in which they do arise. A curious thing is that in the diseases I have mentioned, the tubercle bacillus is very often found to be concomitantly developed in the blood and tissues, and, these diseases being then shewn to be of a tubercular character, are looked upon further as signs or marks of delicacy of constitution, as the phrase is, in those who suffer from them. It is doubtful, however, or more than doubtful, whether this is a just view. It depends on the definition we give to the term constitution, or what we mean by it. If by constitution we mean resistance, then

it would not be inept or unsuitable to use the term as meaning simply that the child in question had a somewhat low digestive resistance, and that in this sense he had a weak constitution. But if we mean (as we generally seem to do) that the original powers of the person were too weak, that somehow or other and from the very first his stamina was poor and low, and much below the average, it is more than doubtful whether we are justified in using the term constitution in this sense; and for my part I think it is a wrong and unjustifiable use to make of it. The question of constitution and its definition will come up for discussion later. Meantime, let it be stated that it is not suggested that the lymphatic derangements described have created the tubercle bacillus. This would probably be impossible, or, at all events, a most unlikely thing to happen, neither man nor his body, nor the body of any other organism, plant, or animal, being able to create anything. But it does not seem at all impossible, or even unlikely, that, if the tubercle bacillus were in the body, it might be attracted by the increased activity going on in those lymph-engorged parts; and that it might there set up its characteristic actions. Most of us contain in our bodies, most probably, numbers of these bacilli (and numbers of other sorts also), but so long as we are healthy we oxidise them off and take no harm, or perhaps they lie dormant for a time and then die. If ingested into the body by being, for example, eaten in meat or swallowed in milk, we can easily

imagine them lying dormant in the body so long as it is healthy ; but when excess of activity has occurred in any part through the lymph engorgement referred to, we can suppose them finding their way to that part, developing and multiplying there, because they find the pabulum suitable for their growth, and so from that place they may find their way all over the body; and, setting up their characteristic actions, may in this way destroy the body. If this suggestion seems far-fetched to any reader of these remarks, let me remind him of what takes place in agriculture when we treat with basic slag grass land producing no clover, or, at least, so little that it escapes our notice. For some years after grass land has been treated in this way, fine crops of clover are grown where no notable amount of clover grew before, and not only so, but the weight of the grass crop is very much increased. Now the basic slag did not contain the clover seed. Being manufactured in the process of making iron, it was white hot at one stage of the process of its production, and nothing living could survive such a condition. Unless, therefore, the clover seed had been added to the basic slag at some time after its production, it could not possibly contain any ; and, of course, we know very well that this was not done. The clover seed must, therefore, have been in the ground, or it could not have grown. In some way, therefore, the addition of the basic slag, while not containing the clover, modified the conditions of the soil and caused to thrive the

clover which, perhaps, before, managed to live some sort of a weak life, being possibly kept down though not destroyed by the grasses among which it grew, until the addition of the basic slag somehow altered its environment and made it thrive. The precise form of the explanation is immaterial to the argument. What is material is that the addition of a manure not containing clover seed made clover grow where it was practically unknown before. There might be other explanations as to how this occurred. But however this may be, who does not see the close analogy between the facts of clover production in this way and the causing to thrive, in the body of man or animals, the hostile micro-organisms of the tubercle bacillus? That in the case of the agricultural experiment we wished the seed to thrive, and that in the case of the tubercle bacillus we did not, is beside the argument, which is on the other hand to this effect, that when we alter the soil (by manuring land, or feeding the animal body, as the case may be), we alter the conditions or the environment, and so organisms spring up which did not formerly do so, or if they did spring up before, did not come to the maturity of development which they subsequently attained. To account for the original origin of the tubercle bacillus, or of any other organism, is part of the problem of creation, and transcends the wit of man; but to account for the presence of the tubercle bacillus in any given place may or may not be difficult, and as it generally lies within the powers

of human investigation and explanation, may profitably occupy us. To decide whether organism or environment was first, to say whether the hen or the egg was first, whether day was before night or night before day, to declare whether structure was before function or function was before structure, are problems which transcend the powers of human faculty. It is not, however, essential to our happiness or comfort in this wonderful and practically infinite universe, extending beyond the utmost stretch of our imagination in all conceivable directions, that we should be able to solve these mysterious questions. It is enough for us that we should see that organism and environment are co-ordinated and co-related, that as the one alters so does the other, that as the other alters so does the one, to see that hen and egg and egg and hen follow one another in invariable sequence, and evidently have done so for an indefinite time in the past, as we may assume that they will continue to do so for an indefinite length of time to come; that similarly day and night follow one another in invariable succession, and form part of the arrangements of the universe, so far as we have to do with it; and that structure is co-ordinated with function and function with structure in such a way that, as one alters, so does the other in a corresponding manner. In the same way we may perceive that the tubercle bacillus tends to grow in tissues, whose activity is heightened by the long continuance of inflammatory

blood congestion and lymph congestion in them, whatever may have been the original way in which its existence may have been brought about; and this knowledge carries with it, or may carry with it, the practical conclusion that we may be able to prevent the growth of that and other micro-organisms in our bodies, if we see to it that these bodies, besides being properly treated as regards air and exercises, have also their food nutrition properly managed, by being supplied with neither too much food nor too little, and by being fed neither too often nor too seldom, in conformity with the work and the kind of work which they are called upon to do.

I have in this chapter set forth the anatomical and physiological facts of the circulation of the blood and lymph, which seem to have a bearing on the advice offered in these pages, at any rate to the average townsman and townswoman, that they should rather tend to restrict than to increase their diet. I deal here rather with the theory on which the advice is founded. Later, and particularly in Chapter VIII., the subject is dealt with more from the practical standpoint. Until he reaches that Chapter, therefore, the reader will perhaps be kind enough to let the subject rest, and turn itself over quietly in his mind; and, meantime, I pass on to the consideration of the two great medical paradoxes.

CHAPTER VI.

Some of the Paradoxes of Medicine.

I DO not know that I can introduce the consideration of this interesting and important part of my subject, or perhaps help to clear up, as I hope to do, some of its puzzling confusion, better than by relating the following occurrence. A lady, who is much interested in medical questions, asked me recently this question: "Can you give me," said she, "any unequivocal sign, doctor, of under-feeding? any one unequivocal sign by which I may know that either myself or my children are under-fed?" On hearing such a question the first idea that occurs to one is, how simple is the question and how easy the answer. Thinness, for instance, wasting, attenuation, general depression, languor, lowness of temperature, slowness of pulse, inactivity of function, weakness—all these one would think to be marks so obvious of under-feeding as to cause surprise that the question was asked at all. But, in point of fact, the answer to the question is so difficult—at least it proved to be so for me—that I was obliged to reply, "I cannot mention any unequivocal, or as it might be called, pathognomonic, sign of under-feeding." For the

curious and even paradoxical thing is that all the signs mentioned—the thinness, the wasting, the lowness, the inactivity, the languor—while they may sometimes, no doubt, be marks of under-feeding, and occasionally are so, may also be, and, in fact, when met with in practice, most commonly are, marks or signs, not of under-feeding, but of over-feeding. When men are exposed in a boat after the wreck of their ship, for many days, with only a few days' provisions on board, we know that when saved they will be thin, wasted and attenuated, that they will be weak, that their pulse may be slow (or quick), their temperature probably low, and their bodily functions generally in abeyance. Mark Twain has described to us, from the layman's standpoint, in his essay "How I made my debut as a literary person," how there had been no action of the bowels for twenty and thirty days in the case of men subjected to these conditions. In one case, he tells us, the duration of the constipation, including a period of time after the man was saved, was no less than forty-four days. "Sleep, also," he says, "came to be rare, but the men did very well without it." In one stretch, the captain did not sleep for twenty-one days and nights. The functions of intestinal action and of sleep were in abeyance, or absent, for a very long time.

Now there could be no doubt what the cause was in these cases. The cause was direct starvation, so long-continued, and with consequences so marked,

that, had it gone on for only a little longer time, the men must have died. And consequently, after the men had been saved, the obvious treatment for them, the only rational thing to do, was to have recourse to cautious and, of course, not too suddenly increased administration of food. And this would have to be continued, as, in fact, it was continued, until the men gradually returned to normal and healthy food habits, properly proportioned to the work which they had to perform. All this is perfectly plain and obvious to the meanest comprehension. But we should make the greatest mistake if we were to assume that because attenuation, constipation and sleeplessness were in these cases caused undoubtedly by too little food for a long continuance of time, they are always so caused. And if, acting on this view, we were to recommend a patient suffering in these ways to increase his food, we should or we might find his attenuation getting worse and his constipation and insomnia increasing. Constipation is inactivity of intestinal function, and it may be brought about in two opposite ways, or from two contrary causes, by deficiency of food and by excess of food. In the one case constipation is induced for two reasons, first, because there is nothing to pass—the intestines have no contents, they are quite empty—and second, because, if there were anything to pass, there is no strength to void it. But in the other case, constipation or intestinal inactivity sets in because, there being too much to pass, the intestines become dry and plugged.

Obviously, while the treatment proper to the first form of constipation is to cautiously increase the diet till we get the man on to proper food habits (when his constipation will begin to disappear), the treatment proper to the second form of constipation is to cautiously restrict the diet in the way described in Chapter III. The first form of constipation is caused by direct starvation, and by the direct weakness which it brings on, but the second by causes just the opposite of this; and I have already described the *modus operandi* in which it comes about. Reflection on the causes of the condition also shews the futility of treating such a state by purgatives. These only irritate the bowel, causing an increase of watery discharge from it by increased stimulation and irritation; but when these subside, unless other measures are meantime taken, and particularly unless the diet is restricted, the constipation is not and cannot be relieved. In fact, it is apt to become aggravated, because the irritation of over-action or over-secretion, set up by the purgative, is apt to be followed by a corresponding diminution of activity, or by under-secretion, which makes the original trouble worse. Unless the person so suffering consents to restrict his diet—and, unfortunately, too few persons are advised to do this, or, if advised, unfortunately refuse to do it, so persistent and so widespread and prevalent are wrong ideas on the subject—he cannot get rid of his trouble. And hence we see those frequently recurring cases in

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which persons have recourse to the use of purgatives for many years without being able to obtain relief from their misery. Instances have occurred within my knowledge in which patients have taken aperients every day for 25 years, and even longer, without succeeding in obtaining cure of their ailment. It is, I confess, a standing puzzle and mystery to me how this state of things can continue. One sees persons capable, in other directions of life, of grappling with difficulties, and of adapting suitable means to the end of ridding them of their troubles; but in a case like this it is not so. Day after day the difficulty recurs, the constipation shews no sign of yielding to the means adopted for its relief. In order to meet this, perhaps the purgative is changed, cascara is replaced by liquorice powder, or one form of patent pill by another, or one mineral water by another, *Hunyadi Janos* by *bitter wasser* or *Friederichshalle* ; or the dose is increased. A patient once told me that, beginning with one pill, which had a decided effect, he soon found it necessary to take two, then three, and then four, and so on, until at the time I saw him he was sometimes taking as many as eighteen, and occasionally even this dose had no effect. This had been going on for fourteen years. How any sensible man or woman fails to discover, after say six months, that there is no relief to be got in this way, and how it is that he still persists in futile efforts of this kind, is a puzzle to me. In any other direction in life, he would, when he found one

means fail, try another, but in this he goes on in the most fatalistic way with the same means, or with means of the same kind, although time after time he finds his remedies fail; or, if he obtains temporary relief, the evil soon recurs, even perhaps in an aggravated form. The patient above referred to told me a year after our interview that he had only once taken a dose of an aperient medicine during the preceding twelve months, changes in the diet having effectively cured the constipation, which no aperients could do more than temporarily relieve. But this patient did not hesitate, when advised to do so, to reduce his meals to two a day, and to make some changes in the quality of his diet.

How serious this condition may become, however, and how serious in point of fact it often does become, may be seen, among other instances, in the case of cancer of the bowel, supervening from this cause, the overgrowth of the muscular fibre at some particular point in the bowel becoming greater and greater, until at last it becomes so great that it contracts so tightly as to prevent any contents from passing, and an attack of intestinal obstruction sets in, which puts the patient into the greatest danger, and may even cost him his life. Even if he survives one such attack, the causes still continuing, and too much nutritive material continuing to find its way into the blood, and thence to be conveyed to the bowel itself, another attack is sure to occur, followed by another and still another. In the end

what is called malignant disease, and what is at least incurable disease, sets in. The overgrowth, though general over the whole bowel, is usually much more pronounced at one point or place than at the rest, and from this place an aggressive or invasive action sets in in the surrounding tissues, and indeed in the whole intestines, involving them in similar action, so that, even if the chief place affected is cut out or exised, and if the patient survives the operation, other parts have been for a long time steadily prepared for taking on a similar action, they may take it on accordingly, and cure for that person may become impossible. People sometimes say that constipation causes cancer. What has been said explains what they mean; but obviously a truer and sounder statement would be, not that constipation causes cancer, but that the causes of constipation are frequently also the causes of cancer. On both views, of course, the practical inference would have been drawn that if the constipation had been cured, the cancer would not have occurred; but on the former view, the futile plan of attempting to cure the constipation by administering purgatives, would probably have been had recourse to, while on the other view, the much better and much more hopeful plan would have been adopted of restricting the diet. The discussion of the causes of constipation has led to a short digression as to how constipation is often connected with incurable disease, like cancer; but the statement I wish to emphasize, and which I think I have proved by

probable evidence, the best sort of evidence which can be brought to bear on the case, is that opposite causes acting on the body may and do induce the same effect. Constipation, obstinate and lasting as we have seen (illuminating and rendering much more instructive the layman Mark Twain's perfectly correct description), for as long as 44 days, may arise from too little food; and it may also be caused, as in fact it far more commonly is, by too much.

It is not quite relevant to the phase of the subject under discussion, perhaps, but this seems nevertheless a suitable place to introduce another consideration bearing on this subject. The person referred to as having taken all those pills told me that his mother had died of cancer. From inquiry it appeared that it had come on with prolonged and recurring attacks of constipation, which it had been fruitlessly attempted to relieve by administering purgatives. Now suppose that my patient had also gone on in the same way; suppose that he had persisted in attempts to cure his constipation by efforts which, in the nature of things, could not have been successful, and supposing that as a consequence he had got cancer also—well, we should have been apt to say that cancer was hereditary in that family. And yet no more would have been proved than that, like causes having acted on like organisms for two generations, like results had been produced. Organisation is transmitted, no doubt, but disease hardly or very rarely indeed. And in the case supposed there would

have been no more proof of the hereditary transmission of cancer than there would be for the belief that death by gunshot wound or rifle bullet is hereditary in a family because son, father, and grandfather, having been military officers, were, one after the other, shot in action. If the son of the last becomes a wool merchant, the probabilities are that he will die in his bed. And if my patient treats his constipation differently from the way in which his mother treated hers, there is no reason why he should have cancer like her.

I have said that probable evidence is the best sort of evidence that can be brought to bear in cases of this sort. The reader may demur to this, and may ask for experimental evidence. Well, what better experiment could be devised than the experiment of civilised nature, as it may be termed, of starving men in a boat for many days, if we want to find out one of the causes of constipation? And, on the other hand, what better experiment can we desire as to the causes of the ordinary and common form of constipation than watching the effects of the food habits of ourselves and our friends? But if minds are so constituted as to be dissatisfied with this kind of evidence, let them, if they are so minded, and if they can get persons to submit to the experiments, subject their friends (or, better still, themselves), to a long course of fasting on the one hand, and to a long course of too frequent and too abundant feeding on the other. Constipation will arise in both experiments. They will find that

opposite causes induce (apparently) the same state. Let them weigh, and measure, and time the quantities of food administered, let them note its qualities and kinds as well. Then they will be able to conclude that certain quantities of food of certain kinds, administered at certain times, will, as a rule, and within certain limits, induce constipation, but that the powers of resistance of different individuals, and of the same individuals in different circumstances, will vary very much in the manifestation of the effect. The net result will be the view that the quantity and quality of food, and the times of its administration which will induce constipation, cannot be stated to a nicety, but that, on the whole, frequent and abundant feeding tends to induce it. In fact, the evidence to be obtained from the artificial experiment will coincide with that from the natural experiment or experiment of nature, which, if we had carefully and, as far as possible, accurately observed it, would have led us to the same conclusion. And a general rule would arise, the same as observation has already led to, viz., that the quantity, and quality, and frequency with which foods would have to be administered in order to induce constipation cannot indeed be stated to a nicety, but that taking it too much and too often is a very potent cause. And of course from this rule would arise the same canons of treatment, the practical conclusions, the things we care most about, the canons which have already been stated. The results of experimental evidence will harmonise

with and corroborate the conclusions of natural evidence, but both are really experimental if properly viewed.

But we may go a great deal further than this. Not only is constipation induced by two opposite sets of causes by too little and by too much; so are *all* conditions in which functions come to be in abeyance. Mark Twain tells us how the captain in charge of the boat for those 43 days did not sleep for 21 days and nights. In fact, through direct starvation, he lost the power to sleep. But how unwise to conclude from this natural experiment, so to call it, that the commonest way in which sleeplessness or insomnia is brought about is by taking too little food, and taking it too seldom. The commonest form of insomnia is, on the other hand, caused by taking food too often and too much. The explanation of the *modus operandi*, in which the same result (I use *opposite* in the sense of *contrary*, not of *contradictory*, *i.e.*, as marking differences of quantity and frequency, as to more or less, but not as marking differences as to *kind* or *essence*—which would be termed *contradictory*. Day and night, heat and cold, *e.g.*, are contraries of one another, but motion and rest—if there *is* such a thing as absolute rest—would be contradictories), is brought about by these two opposite conditions, may be difficult, and different opinions may be held about it. I think the explanation is the following, and that it is the same in principle as the explanation

already given of the induction of constipation by the two opposite causes of too much and too little. In order that sleep may be induced the circulation of the blood (and of the lymph) in the brain must be moderate and proportional. But the function of food is to make blood and lymph, and through proper stimulus of nerves to control their circulation. When men are directly starved for many days, so that they threaten to die from direct starvation, they, of course, use up the nutritive powers of their blood. Blood too poor in nutritive materials is sent again and again to the brain and other organs. In normal circumstances this blood stimulates the coats of the arteries to contract and help in assisting the passage of the blood along the arteries, and also keeps these coats in proper tone. In normal circumstances, it seems to me, that through the action of the trophic or vasomotor nerves the normal tone is maintained in these coats, as also a normal proportion between the action of the longitudinal and the transverse or circular coats of the arteries. In health I think the arteries are contracted in sleep, while the veins are dilated; but contraction is kept up by over-stimulation of the circular coats as compared with the longitudinal coats. Normally I think healthy balance between these two, results in rather more stimulation of transverse than of longitudinal elements.* It may

* The same relation obtaining between the longitudinal and transverse muscular fibres of the bladder and bowels respectively, leads to the prevention of unpleasant accidents in their corresponding functions.

possibly be that, while both the longitudinal and the transverse elements are under the control of the nervous system, the transverse coats are rather under the control of the sympathetic system of nerves, while the longitudinal are rather under the government of the cerebro-spinal system. If so, this would account for the well-known fact that too much excitement or too much anxiety causes a feeling of throbbing in the head, and prevents sleep. By the suggestion made, or in accordance with it; excitement, and anxiety, and mentalisation, or cerebration in general, tend rather to stimulate the longitudinal elements of arteries, and so, by contracting them, to shorten and widen or dilate the vessels, and so, by over filling them with blood, and hence by supplying too much blood to the brain to keep it awake, and prevent sleep. Too much food often has the same effect, If the effects of over feeding continue, a compensatory action is set up, and the transverse coats of vessels increase in size or they hypethoply, as if in order to overcome the overplus of stimulation of the longitudinal elements. And, in fact, we find in gout and other states of over feeding, great increase in these circular or transverse coats, so that, in some cases, they contract so tightly as scarcely to allow the blood to pass at all. In this case their action must be to induce a sort of obstipation or constipation of the brain vessels, just as the corresponding state already referred to in the bowel narrows its lumen so much as to allow nothing or

hardly anything to pass, and so to cause constipation of the bowels from excess of food. But in the opposite condition, that of direct starvation, acting for a very long time, the blood has parted with so much of its nutrient material that it cannot stimulate the coats to act at all; and so, as in the analogous case in the bowel, whereas we saw there was nothing to pass, and no strength to void it if there had been, in this case the powers of the brain and nervous system are so weak through the impoverishment of the blood supply that the nervous system is unable to sleep. Here is, therefore, another function, that of sleep, which disappears under starvation, and disappears also under the long continued action of too much food, and this, be it observed, even if the explanation I have suggested of the *modus operandi* is incorrect. In any case, the practical conclusion that we come to is that the ordinary form of insomnia ought to be treated by cautious restriction of the diet. If a man is sleepless on four meals a day, let him try three for a while; if this is insufficient to make him sleep, let him take two; or, if two are still too many, let him try one daily meal. But as we cannot wait so easily for the arrival of sleep as we can for the action of the bowels in prolonged constipation, an admirable device for procuring sleep is often found to be either starvation or a very restricted diet, a glass of milk, *e.g.*, morning and evening, or a cup of cocoa made with milk, with a basin of soup with, say, a slice of bread, in the

middle of the day. This diet is in most cases effective in inducing sleep in ten, or twelve, or fourteen, or twenty days. If at the commencement of treatment we think it right to administer a sleeping draught once or twice, it is only in order that we may be able soon to cease its administration; for, bad as are the effects of administering purgatives to cure obstinate constipation, much more damaging and much more to be deprecated is the too common practice of attempting to procure sleep by means of frequently repeating the administration of hypnotics. I hope, in fact, that the reader is beginning to be prepared for the statement that he is a poor physician (whatever be his legalised medical qualifications) who shall attempt to cure constipation by purgatives and sleeplessness by the administration of hypnotics. But not only so; I hope he will be beginning to see that the statement may be generalised, and that he will perceive how poor is that form of medical practice in general which shall attempt to combat any long-continued condition in the body by the administration of remedies calculated to induce the opposite state—which shall attempt, *e.g.*, to cure long-continued diarrhœa by administering astringents, lowness or depression by administering stimulants, and stimulation or excitement by depressants, excess of acidity by the administration of alkalies, or excess of alkalinity by the administration of acids. These conditions may possibly be combatable in this way if they are not

of long standing, for the sufficient reason that in this case they tend to get well of themselves, and even without treatment; but if they have existed for some time, such treatment is quite faulty, and persistence in it cannot eventuate in the cure of the patient, unless the causes of the irritation are also at the same time reduced, the usual translation of which is that the diet ought to be restricted. Of course, if the irritant were lying in the digestive tract, administering a purgative might succeed in removing it; but if it were in the blood, no such means could succeed in doing so. Hence the futility of administering purgatives even where patients are plainly and avowedly suffering from the long continued effects of over-feeding. A purgative cannot remove the cause from the blood, though it may succeed in doing so in the case where some irritating material is lying in the stomach or intestines. The same indeed is true of blood-letting, for which, however, there is really something more to be said, for bleeding, while it may succeed in removing a portion of the *materies morbi* in the blood, can, after all, remove only say half a pint or a pint of blood, while the quality of the blood which remains is unaltered, and must continue to exert its deleterious influence on the economy; and, if the over-feeding which led to the blood containing too much nutrient material in it is continued after the bleeding, what can we expect except that the continuance of the causes will re-induce the old effects? For this reason, among others, bleeding

has been practically given up by medical men. Let us hope that we shall soon see the evil effects of purgatives and hypnotics as clearly. While insomnia due to over-feeding must, therefore, be treated by restriction of the diet, that form which is due to direct starvation, the form from which Mark Twain's men were suffering, must be treated by cautious feeding and cautious increase of the diet.

The ancient writers on medicine used to say that diseases were hot and cold or moist and dry, and that the indication for treatment was, therefore, to oppose dry by moist and moist by dry, to oppose hot by cold and cold by hot. Stated in these ways these terms seem fanciful, as, in fact, frequently was the treatment adopted in conformity with them, remedies being likewise divided into dry and moist and hot and cold. But if we translate these terms into the scientific language of to-day, we shall see that they were perhaps not so fanciful as we had supposed, or, at least, that solid facts underlay them. For, undoubtedly, in some diseased conditions, although the ancient physicians could not measure it, the temperature of the body is too high, and in others it is too low, and the physician often enough attempts to diminish too high a temperature, say, by putting an evaporating or cooling application on the head, or even by administering (though this is generally not so wise a measure) a remedy whose effect is to lower temperature. To elevate too low a temperature, which is almost always caused by too much food, is a

much more difficult thing, and to attempt to do this by stimulants, for example, is to end in failure, the only sound methods of doing so being through restriction of the diet and by recommending exercises with or without the use of baths. But reflection shews that though it might have been fanciful to speak of hot and cold diseases, it is not at all so, but is, on the contrary, highly practical to speak of those with a temperature above natural and those with too low a temperature. Likewise, moist and dry in disease are often described nowadays by the words, too dry and burning a skin, or one too moist and sweating; and physicians to-day frequently recommend, at least, as temporary measures of treatment, sudorifics, *e.g.*, to make the skin act; or they recommend the body to be sponged over with say tepid applications of weak vinegar solutions to check sweating; or even administer remedies with that intention. Of course it is much sounder and better practice to inquire what made the temperature too high or too low, what made the skin too dry or too moist, and to take remedial measures in accordance with our conclusions, than to rest contented with opposing moist by dry, or hot by cold.

These reflections have, however, brought us in sight of our second great paradox in medicine, which is that the same causes often induce opposite states in the body. The first, of course, was that opposite causes induce the same state. Under the second paradox we find statements of this sort:

the causes of constipation and of diarrhœa are often the same, viz., excess of stimulus, the chief form of which is excess of food. The causes of feverishness or elevation of temperature, and of depression with too low a temperature, are often the same, viz., wrong food habits; so with the causes of too great dryness and of too great moisture of the skin; so with the causes of too heavy sleep and too little sleep—they are often the same. And of course it follows, if this is so, that the treatment of these various and opposite conditions must frequently be the same, the great remedy being restriction of the diet. Much consideration and much patience is required before assent is given to this general proposition that the correct treatment of opposite states is the same. At first sight, in fact, it seems impossible that it can be true. To recommend an obese person to restrict his diet or his drink may seem quite rational and in accordance with common sense and common experience. But to say to a thin, wasted, weak and attenuated person, "you must restrict your diet in order to get rid of your thinness, your wasting, your attenuation, your weakness;" this, it must be admitted, does sound foolish and unnatural. Nevertheless, it is so often true, and so often helpful to the patient to offer him this seemingly parodoxical advice, that we must consider and patiently discuss it for a while, because unquestionably, he who does not understand it can have no correct view of medicine and medical

practice, and will, in consequence, fail to cure a large proportion of patients, whose ailments, nevertheless, are curable if the treatment is properly set about. But we shall find, if we allow ourselves to observe a little, and to reflect on what we do observe, much corroboration of this view. Breeders of fowls, for example, and fatteners of fowls for the market know that if animals are over-fed they become thin and wasted, and fall off in weight. The mode in which Sussex and Surrey fowls are fattened for market is said to be the following. For about a fortnight they are confined in coops and supplied with as much corn as they can eat. After about that length of time, their appetite falls off, whereupon, for another fortnight or so, they are artificially fed with soft food, inserted into their gizzards with a squirt several times a day. They have then reached their highest weight, and it is found that they must then be sent to market, if the owners are to make the best profit out of them, because, if they are not, they then, or soon, begin to fall off in weight, to become thin and attenuated from over-feeding. The first effect of over-feeding is greatly to increase the weight, but the second as markedly to diminish it. The same cause, viz., over-feeding, does induce opposite states, viz., too great stoutness and too great thinness. The thinness which follows over-feeding was called by Dr. King Chambers the starvation of over-repletion. It is quite evident that a sound way to overcome it is and often must be to restrict the diet.

But it is not difficult to see how this may be so, because, in fact, people themselves will often come to doctors with the statement: "My food does not seem to be doing me any good, doctor." And the doctor has frequently to reply, "You will get more benefit out of it if you take less of it." No doubt this state of thinness and wasting is often treated by over-feeding, and apparently with good results, at least for a time, patients improving very much under seclusion and over-feeding, as it is called. But do not let us be led away by words. Let us see what this diet by "over-feeding" really is. Here is an account of the diet of one of these cases, variously termed hysteria, neurasthenia and anæmia, in which patients are thin, low and weak, and in which they are treated by over-feeding.

"Patient kept in bed; rising only to relieve calls of nature.

"First day: one quart of milk in divided doses every three hours.

"Second day: cup of coffee on awakening. Two quarts of milk in divided doses every two hours. Aloetic pill at night.

"Third to sixth day: same diet.

"Seventh, eight and ninth days: same diet, with a pint of raw soup in three portions (the soup is made by treating raw beef with strong hydrochloric acid).

"Tenth day: 7 a.m., coffee; 7-30 a.m., half a pint of milk; 10 a.m., 12 noon, 2, 4, 6, and 10 p.m., ditto; soup at 11 a.m., 5 and 9 p.m.

"Fourteenth day: egg and bread and butter added.

"Sixteenth day: dinner added and iron."

But in what sense is this diet a generous diet? It is not generous at all. It is a spare diet, yes, and a very spare diet. A quart of milk in the first 24 hours, and nothing else, surely that is a very restricted diet. In calories, it comes to about 600, while the fasting man, as we shall see in Chap. VIII., on the authority of a distinguished physiologist, requires, or at least produces, 2303 calories. But the first day is only the introduction. Let us see the next stage. From the second to the sixth days double this quantity of milk was administered, and nothing else. But 1200 calories, the amount of energy obtainable from this diet, is yet only a little more than half of that given out by the starving man, and about half of that emitted by the physician or the official, as we shall see in due time. Up to the present point, therefore, this diet of over-feeding is only about half of a starvation diet. From the seventh to the ninth days a pint of raw soup was added, *i.e.*, a pint of soup into which the juice of a pound of raw beef had been strained. This would not and did not contain much of the nutritive matter of the beef, but if we were to admit that it contained all the nutritive value of a pound of beef, and were to add 1000 calories as representing the energy value, we should still find that our so-called over-feeding diet amounted to only 2200 calories of energy value, which is even yet less than

the amount demanded by the starving man. But it would be a gross exaggeration to suppose or estimate that a pint of raw soup, made in the way described, contained anything near 1000 calories. I really think that 100 calories would be nearer the energy value of a pint of raw soup made in this way. We come, therefore, after what I think is a very fair and quite impartial examination of a diet of "over-feeding," to the conclusion that for the first fortnight it is a diet of restriction, and even of great restriction. I quite agree, nay, I strenuously assert that the principle of treatment by restriction is correct; but one must emphatically protest against its being called a generous diet or a diet of over-feeding. The patient's body is being called upon during that fortnight to part with its surplus stores, those surplus stores which made the patient weak, thin and attenuated, because they were in excess. This will be appreciated better if it is realised that the urine passed in these circumstances has often a specific gravity as high as 1030 in place of 1010. A most excellent canon of treatment, in fact, for these cases is to go on with the greatly restricted diet until the specific gravity of the urine falls to 1010, after which cautious increase of the diet may be ordered, especially if in the meantime the very low bodily temperature generally found in these cases has shewn signs of rising. In four to six weeks or so in my experience, these two facts begin to shew, when I generally get patients on to one solid meal a day, and continue that for some

time, and until they are ready for more, and until they demand more.

But let us pursue the account of the over-feeding diet, which, for the first fortnight, is, as we have seen, a diet of starvation.

"Nineteenth day: The entire diet was as follows:—7-0 a.m., coffee. 8-0 a.m., iron and malt extract; breakfast, consisting of a chop, bread and butter, a tumblerful and a half of milk. 11-0 a.m., soup. 2-0 p.m., iron and malt; dinner of anything liked, with six ounces of Burgundy or dry champagne, and at end one or two tumblers of milk. 4-0 p.m., soup. 7-0 p.m., malt iron, bread and butter, usually some fruit, and two glasses of milk. 9-0 p.m., soup. 10-0 p.m., aloetic pill." I have had some difficulty in calculating the calorie value of this diet. I am sure it is too much, and that after the patient had been on so restricted a diet for the first fortnight it would have been better to go on as on the sixteenth day, with dinner added to the starvation diet, when the patient would have had sufficient food, and would not have required the aloetic pill at bed time. On a sufficient diet, neither too much nor too little, the bowels regulate themselves, and no aperients are required. If, on the other hand, aperients are required, we may be sure that the food administered is either too much or too little. So far, however, as I can make out, the diet represents about 3445 grand calories in value, made up as follows. The bread given is not quantified, but I have assumed that 4 ozs. were

given at 8-0 a.m. and 7-0 p.m., and 2 ozs. at dinner, that is, 10 ozs. in all. This is more, let me say here, than the average woman can continue to take without getting indigestion, colds, rheumatism, &c. She may of course, however, take this amount daily for some time without suffering in these ways, for as long perhaps as the "over-feeding" diet lasts, though what a misnomer "over-feeding" is in the first fortnight of the diet we have seen.

7-0 a.m., cup of coffee. There is no calorie value in this, or almost none. Its use in the body is, however, great, as it stimulates the body to draw on its own reserves and to use up some of them. The increase of temperature which generally follows the administration of a cup of hot coffee is a proof of this. I have known the temperature of the body to be raised by one or two degrees F. for an hour or longer by such a dose of coffee. Half a pound or half a pint of hot coffee solution at 110° F. could not possibly raise the temperature of a woman weighing about 100 lbs., through one or two degrees F. in temperature. It is a physical and mechanical impossibility. But it may and easily does so by stimulating the body to oxidise some of its own surplus stores lying unused, and to some extent clogging the body; and so it performs a very useful part in the economy.

8-0 a.m., breakfast. 4 ozs. of bread = 276 calories, and 4 ozs. of chops uncooked = 3 ozs. cooked = say 120 calories, and milk = 225 calories, butter, 1 oz. = 208 calories.

11-0 a.m., soup. This contains little or no calorie value, acting in much the same way as the hot coffee does, and stimulating the better digestion of the breakfast so as to prepare the stomach for dinner at two. But let us say that half a pint of soup contains 65 calories (perhaps 50 calories would be nearer the mark).

2-0 p.m., ordinary dinner. Suppose this to weigh two to two and a half pounds of mixed diet as it comes to the table, made up say of 4 ozs. of meat = 196 calories; 2 oz. of bread = 138 calories; $\frac{3}{4}$ lb. of potatoes = 270 calories; cooked fruit, 1 lb. calorie value, say 200 calories; 1 oz. of cheese = 125 calories; 1 oz. of butter = 208 calories; two tumblersful of milk = 300 calories.

4-0 p.m., soup = 65 calories.

7-0 p.m., bread, 4 ozs. = 276 calories; butter, 1 oz. = 208 calories; fruit, say 200 calories; two glasses of milk = 300 calories.

9-0 p.m., soup = 65 calories.

The total estimated calorie value of this diet comes therefore to about 3445 calories. I have not been able to do more than guess at the calorie value of the fruit, which, it is well known as to many kinds of it, consists of about 90 per cent. of water; but I have estimated the calorie value of a pound of it as about equal to 3 ozs. of bread. This specimen of diet of over-feeding, taken from Professor Davis's Dietotherapy, can scarcely be termed a very excessive diet, representing in calorie value barely more than half as much again as the

energy lost daily by the starving man, but with the patient in bed is probably a good deal more than was required. For one thing, the need for administering an aperient at bed time is itself a suggestion in this sense. But this specimen of a diet of over-feeding is much less than is administered in some cases in which patients are heavily fed every two or three hours during the day, and in which much larger quantities of food, representing a much higher and, it must be held, quite unnecessary and very undesirable quantity of calorie value, are given. Even if apparently well borne at the time (but many patients rebel greatly when undergoing the treatment), it is difficult to avoid the conclusion that later it must lead to the occurrence of gout, rheumatism, colds, headaches, or other ailments in patients so treated, especially as the treatment itself offers a tacit suggestion that the patient should go on afterwards living more or less under the idea that heavy feeding is a main means towards maintenance of health and strength.

The explanation of the high specific gravity of the urine formerly referred to is simple, on the theory that the body and its circulation, the blood and the lymph spaces, are loaded with unused material accumulated within them, and on the view that, under restriction of the diet, the body begins to use them up, and to eliminate them; but it is difficult to explain it on any other theory. In fact, if we examine we shall find that the urine passed by persons in the state of neurasthenia, ovarian

neuralgia, hysteria, general debility, and wasting without feverishness, for the treatment of which recourse is generally had to what is called over-feeding, differs from time to time very much in specific gravity. Sometimes a large quantity of urine looking very much like pure water is passed. This, on examination, proves to have a very low specific gravity, say of 1004 or 1006, and to contain very few chemical salts. At other times, and more often, the urine is found to be of the high specific gravity of 1030 or 1032, formerly described. When this occurs under restriction of diet, and before over-feeding is commenced, a possible explanation might be that the body is living on itself, that it is using up its own materials, and that the high specific gravity is caused by the presence in the urine of the products of the oxidation of the tissues of the body. If this were so, however, to any great extent, feverishness, or elevation of temperature, with quickening of pulse, might be expected to be present. As a rule, however, neither of these conditions is present, the temperature being generally considerably too low (96° or 97° F., *e.g.*, in place of 98·4° F.), while the pulse is not quickened. Still, the body must to some extent be considered to be living on itself during the treatment, because, during restriction, it is not being supplied, as we have seen, with sufficient food for the long continued maintenance of life; and we are corroborated in this view by the fact, frequently observed, that women treated in this way

may increase their weight at the same time that their food is diminished. This has occurred frequently in my practice, and, among other cases, in that of a young woman, twenty-six years of age, who had been ill for three years with dyspepsia and anæmia, and had been unable to follow her occupation for two years. For the first four weeks of treatment she took a tumblerful of milk mixed with an equal quantity of boiling water morning and evening, and about half a pint of mutton-or chicken-soup in the middle of the day, and nothing else. She was not confined to bed, but went about all the time. For the next five weeks she had the same allowance of milk morning and evening, or she took hot barley water in place of the hot water. For dinner she had half a pint of any soup with some green vegetables in it, well cooked, as sprouts, cauliflower, or celery, or cucumber, and some Parmesan cheese dredged over it. The solid constituents of her diet did not weigh more than eight ounces, and green vegetables contain, it is well known, about 90 per cent. of water in their composition. Nevertheless, on this diet, she got entirely rid of her dyspepsia, no longer vomited the bitter stuff she formerly did, and, to a great extent, got rid of her anæmia; her friends complimenting her on her improved appearance. More strange it is to add that on this diet she gained one and a half pound in weight in three weeks. No doubt she drew on the over-accumulation of reserves in her body for some of the supplies on which she lived during the time, for it

is not to be supposed that she could continue to live on such a diet, especially after she shall have returned to her work. And the waste, effete, unused material accumulated in her body, and converted under this regime into the heat and energy of life, was probably replaced by water in the tissues, so that she gained weight. A certain amount of this replacement by water was no doubt of the greatest benefit to the patient, for in anæmia the tissues are so shrunk and "constipated," or obstipated, and the circulation in them is so blocked, that the blood cannot enter them freely. The oxidation and metabolism, therefore, that ought to occur in the tissues is prevented from taking place. This leads to further blocking and further obstipation, especially if attempts are made to "feed up," as they so frequently are in this affection. The first part of the treatment which goes under the name of over-feeding being, therefore, a restriction of the diet, so that the body may be called on to eliminate the excess of material accumulated within it, the next part of the treatment ought to consist in getting the body on to proper nourishment, and this the treatment attempts to do. Of course much depends on the view which we take of the facts before us. If we think that the lowness, the weakness, the thinness, and the low temperature are marks of under-feeding, we shall order a course of gradually increased feeding, or even, perhaps, for a time, of over-feeding. If, on the other hand, we think

these are the sign of plugging, blocking and obstipation of the fluids and tissues of the body, we shall restrict the diet. It is a curious thing to find that the first fortnight of what is called over-feeding is really a diet of great restriction. But whatever view we take of the causation of the patient's state, there is certainly a danger that in the next part of the treatment the administration of food may be carried so far as to over-load the blood, and lymph, and tissues with excess of the products of digestion; and it is to me, I must say, more than doubtful whether the greatly increased diet sometimes administered, a diet much heavier than the one described by Professor Davis, is either necessary or desirable. It seems to me to cause loss of strength, plugging, blocking, constant tendency to take colds, headaches, feeling of fatigue, and other ailments. Some patients even resent it while feeling over-persuaded to submit to it; and I have heard complaints from such that they were simply stuffed like geese that are being prepared for the market.

We have been led into this digression through consideration of the different methods of treatment that must be adopted according to the different points of view from which we see the facts before us, and because this seemed a suitable place in which to discuss the treatment of over-feeding. We must now proceed to discuss further the aspects of our second paradox that the same causes induce or cause the production of cpposite states in the

body. This academic discussion has the most important practical bearings, as we have seen. It makes to the patient all the difference between recovery and failure to recover, between life and death, what view we take of his ailment, because on the view we take depends the course of treatment which we recommend for him. An academic discussion, therefore, on the part of the medical adviser is a practical matter of life or death to the patient. If the medical adviser or the reader is tempted to think, he will steer clear of the whole discussion, if he thinks he will give it the go-bye, if love of peace and quiet tempt him to withdraw and let others fight it out, he must be reminded that peace is not so to be obtained, that refusal to consider a question does not make it non-existent, and that the question is a clamant one and of the most supreme importance, because his future and that of those dependent on him is bound up in it. He simply must face it, for there is no escape.

It is difficult to find the best way of putting the results of the second medical paradox. Perhaps as good a way as any to explain how constipation and diarrhœa, slow pulse and quick pulse, depression and fever, melancholia and excitement, with a variety of other opposite conditions, are frequently produced by the same causes, and particularly by excess of food, is by stating the following proposition. ALL AGENTS WHICH AFFECT THE BODY OR ANY PART THEREOF EXERT

ON IT A TWO-FOLD AND CONTRARY ACTION IN TIME, THE SECONDARY OR REACTIVE ACTION BEING THE OPPOSITE OF THE ACTIVE OR PRIMARY ONE. As we have seen, there are only two fundamental states in the body, or in parts thereof, or tissues thereof, viz., shrinking and swelling, contraction and dilatation, *strictum et laxum*, as the Latins called them. The Greek medical writers knew these two conditions under many and varied names, as, *e.g.*, *ἀτονία καὶ ῥῶσις; στεγνὸν καὶ ῥοῶδες; στέγνωσις καὶ ῥῦσις; τάσις καὶ χάλασις; συναγώγη καὶ χύσις; κεκλεισμένον καὶ ἀνεωγμένον; πύκνωσις καὶ ἀραίωσις*. The Latins, again, keeping, in accordance with the genius of their race, rather to the simpler and logical division of *strictum et laxum*, than to the more florid and more imaginative subtleties of the Greek mind ; but finding that discharges (or as we should now term them, secretions and excretions, especially the latter) are given out from the body during the state of *laxum* or dilatation, unfortunately introduced another term, viz., *solutio*, as the opposite of *strictura*, and then by a lapse of the logical faculty came to use *solutio* for any condition characterised by such discharges as phlegm in coughing, or diarrhœa or sweating, instead of as a name for the dilatation which they thought to accompany them. By opposing *strictura* to *solutio* they really mixed up property and function, since *strictum et laxum* are fundamental properties of organised (and unorganised) bodies, while of

strictura et solutio, the latter at least denotes a functional change occurring in an organised body, and, as such, ought to have been opposed not to *strictura* but to *absorptio* or *imbibitio*, if there had been such a word. The logical and word-saving Roman became confused here, but he was right to this extent that in *solutio*, when some material is being expelled from the body, the process is accompanied, no doubt, by dilatation of the longitudinal elements of tissues, but is effected by contraction (*strictura*) of the transverse. *Absorptio*, on the other hand, takes place only when contraction (*strictura*) of the longitudinal elements occurs, and this, as we have seen, necessarily leads to dilatation (*laxatio*) of the transverse, the latter action causing opening of the mouths of vessels, by which alone absorption is possible. In *strictum* of the transverse elements, which is, of course, accompanied by *laxum* of the longitudinal, organised bodies, having expelled their secretion or excretion, become dry; while in *strictum* of the longitudinal elements, leading necessarily to *laxum* of the transverse, moisture appears in organised bodies. *Strictum et laxum* therefore are the physical or anatomical changes which occur in tissues; *absorptio et solutio* are the corresponding functional changes. But as the ancients, both Greek and Latin, had not distinguished either between structure and function on the one hand, or between the necessarily opposed states of contraction of longitudinal and transverse elements on the other, and, as lastly they did not

clearly see that *strictum* of elements is invariably in time followed by *laxum* of the same elements, their thinking on the subject became very much confused. For, when the Latins spoke of *stricturæ et solutionis complexio* they plainly mixed up contraction of longitudinal and contraction of transverse; whereas, had they used the term *alternatio,* and had they spoken of *stricturæ et solutionis alternatio,* of alternation in place of combination or mixture, they would have kept to simpler considerations, which would have delivered them and their followers from much confusion. When, however, contraction of one set of elements has the same effect as dilatation and even paralysis of another, things do become very complicated and difficult to explain. From this point of view we may perhaps somewhat understand how medical expert A advises his patient in a sense diametrically opposite to that recommended by his colleague B, the permutations and combinations of two or three pairs of opposite things being almost impossible of comprehension by the average human intellect. And we must to this add the consideration that a vehement desire on the part of the patient to escape from his misery, and a failure to discover whether the misery depends on excess or deficiency, or rather perhaps the conviction that it does depend on too little and not on too much, is apt to force the hand of the medical adviser and to compel him to allow indulgences which his judgment feels are beyond the line of true moderation. Our general

proposition, therefore, translates itself into this, that when an agent primarily shrinks the body it secondarily swells it. Cold is such an agent, and the effect of cold is such that while its primary action is to shrink the tissues, its subsequent action or its reaction is to swell them. Shrinking, therefore, or swelling, may either of them be the effect of cold. Heat also first contracts and secondly expands the body, and so either contraction or swelling may be the effect of heat. Contraction of transverse elements may be considered as the type of depressing action ; their dilatation, on the other hand, as the type of elevating or feverish action. Tonics usually act as primary shrinkers and secondary swellers. Relaxants usually act as primary swellers and secondary shrinkers. Exposure to cold first braces up to action and then causes swelling, fatigue, and sleep—after which we are ready to begin again. Alcohol, on the other hand, first reddens the face, relaxing the vessels, and then shrinks them, so causing pallor, and, if the action has been carried far, shrinking secondarily the vessels of the brain so much as to cause a headache in the morning. As a small quantity of the agent which in excess did the mischief, and which caused the headache as its secondary effect, relieves the headache by dilating the vessels as its renewed primary effect, we see how much room there is for difference and even for opposition of practice, and perhaps we may be able to understand somewhat the reasons for the existence of rival schools

opposed in practice to one another. But a patient and repeated investigation of facts, and the explanation of the same, must unquestionably lead to greater toleration by rival schools of the methods of one another, and in time, perhaps, to better agreement among them as to practice. How much depends on the natural action of nature, and how much is to be attributed to the artificial action of the remedies administered, ought reasonably to be considered by both sets of practitioners. The result of such inquiry, and of others like it, will undoubtedly be good both for the practitioners and for those on whom they practise. Take the case of a navvy working in a sewer with his feet all day in six inches of water, perhaps foul water, and a wind blowing through the sewer. The man's tissues are shrunk by the application of cold and by the loss of heat from his body. He shivers, and (especially when there is much effete unused stuff in his body, that is, when, as is mostly the case, he has not assimilated all the food he has taken) he is soon found to have a high temperature and quickish pulse, and may be diagnosed to be suffering from pneumonia or inflammation of the lungs. Cold was the immediate exciting cause of this. The predisposing cause was the state of the man, particularly the relation of his body to the prolonged influences of its food environment. But an effectual mode of treatment for the man might be—it is often the most effectual of all—to envelope him in a wet sheet pack for an hour; to restrict his diet, so that

he shall use up some of the unused stuff that is in him; and to renew the application of the pack at intervals till his temperature and pulse fall, and the exudation into the oppressed lungs is resolved and eliminated from the body. Here excess of exposure to cold makes a man ill, and judicious exposure to moderate and quantified amounts of the same agent makes him well. If, now, instead of taking the case of what is called acute illness, the case in which the exposure to the cause acts suddenly and severely, we try to imagine the slower action of the cause, when it is continuing to act for a considerable length of time, we shall get some light on the subject. Suppose a person to be exposed not to sudden and severe cold, but to slighter amounts of it acting for longer periods. Suppose also that the body is subjected to a slow but not at any time excessive condition of over-feeding, so slow, indeed, that the body may be made gradually to be tolerant of the irritation. We can imagine (what frequently happens), that slowly the body is depressed, and that its powers are lowered, that the pulse may fall, and the temperature may be diminished, till they seem to permanently shew depressed readings. If acute or sub-acute attacks of illness, repeated "colds," for example, or repeated attacks of influenza, supervene, as they often do, the tone of the body is so much lowered, it is so much depressed, and the action of the depressing agents, the cold and the over-feeding, are

so slow and gradual, that even the reaction of the sub-acute attack is not great. The patient has only a slight elevation of temperature, to 99° F. or 100° F., say, after the subsidence of which, it falls down again to its former level of 96° or 97°. It seems that it is quite easy in this condition to mistake the effects of too much for the effects of too little. This is an example of our first paradox (since depression may be caused both by too much and by too little), that opposite causes induce the same state. But further examination also shews that the depression alternating with the feverishness are both effects of the same causes, and therefore it shews the action of the second paradox, that the same causes induce opposite states. We may put this in another way, and say: irritation with tolerance causes depression, while irritation with intolerance causes elevation or fever, or at least feverishness; but both depression and elevation, both a too low temperature and a too high temperature, are the effects of long continued irritation. In point of fact it is far rather the long continued improper feeding, the long continued excess of irritation, which is the main cause both of the depression and the feverishness, than the prolonged exposure to cold, so that, if a person is properly fed, neither taking too much on the one hand nor too little on the other, he will not be put about or made ill by any moderate exposure to cold such as he may experience in the ordinary conditions of English or

European life. I think, however, that I have shewn not only that, in accordance with paradox I., opposite causes induce the same state, but also that, in accordance with paradox II., the same causes induce opposite states, and that these opposite states are apt to alternate with one another. If we think of it we shall find that the frequent alternation of depression with feverishness, of too low a temperature with one too high, might be, and often is, termed irregularity of action. And from this it will follow that a constant or long continued irritation, acting on the body, induces not only deficiency of function and excess of function, but also irregularity of function. And from these considerations arises also the practical conclusion, so important it seems to me, that in the medical management of the body its importance can hardly be exaggerated: If you find defect of function, irregularity of function, or excess of function, look for a constant cause, or at least for one acting at very short intervals over a long period of time. As by far the most important cause is improper nutrition, and as the form which this takes is far oftener over-nutrition than under-nutrition, the practical conclusion that emerges is this: If you find yourself suffering from defect of function, from irregularity of function, or from excess of function, look for a frequently acting cause, and recollect that most frequently (perhaps three times out of four, or five times out of six, or oftener), that frequently acting cause will be found

to be too much food. Next to that, probably, it will be found that improper relations between the body and air are the chief cause of illness (for illness is either defect, or irregularity, or excess of function). Next to the relations of the body to air, probably, it will be found that those to work or occupation will be important, after which a variety of other causes, like anxiety and exposure to various of the exciting causes of illness, will take rank, such, *e.g.*, as damp, cold, moisture, dryness, &c.

This chapter is becoming long, but the importance of the subject is great, and there are difficulties connected with it, some of which I hope are now less in the mind of the reader than when he began to consider them. I do not wish to labour the matter further, so will close this chapter with an illustration from the course of business or trade. There are two conditions in which business tends to be depressed, or even to come to a stand-still. The first is when there are no stocks on hand, for when tradesmen have nothing, they can neither buy nor sell (unless, indeed, they deal speculatively in things which do not exist, which is not legitimate business). But the second condition in which business tends to come to a stand-still is when stocks are so heavy that merchants are afraid to do business for fear that, instead of being able to make a profit, they may sustain a loss. In the former case business is checked by too little, it is starved ; in the second it is clogged by too much, it is suffering from the

starvation of over-repletion. This is the first paradox: opposite causes have induced the same state. We have now to see in illustration of the second paradox that the same causes induce opposite states, how the alternation of boom and panic in business is the effect of a constant and a frequently acting cause in the commercial organism, just as the alternation of fever and depression is the mark of such a cause in the physiological organism. The cause in the one case is an unrestricted desire for gain on the part of the merchant. The cause in the other case is a too unrestricted or ungoverned desire for food on the part of the man, woman, or child. When the desire for gain on the part of the merchant is natural, reasonable, right, neither too great nor too small, then the course of business shews moderate elevations at some periods, alternating with periods of less activity, or of moderate depression at others. But there are no inflated booms on the one hand, and no panic depressions on the other. Let, however, the desire for gain be too great, disproportionate, inordinate, and then we get cornering and other devices introduced, means intended to gain monopoly for ourselves while we are attempting to deprive our neighbour of his fair share of business and of profit. The consequences of this are booms alternating with panics, inflated prices, when things are not worth for life what they are fetching, alternating with times when they do not fetch anything like their real value to life.

We have, therefore, excess of business — too great excess—alternating with defect—too great defect—and the whole marked by irregularity; just as we have feverishness, depression, and irregularity in the bodily functions, arising from too unrestricted desire for food on the part of the man. But just as we may infer from defect of business, excess of business, or irregularity of business, that there is a constantly or frequently acting cause to account for it, and that an unrestricted desire for gain is far the most likely cause to account for these conditions; so we may infer from defect of function, from excess of function, or from irregularity of function in the physiological organism, that there is a constantly or frequently acting cause to account for it, and that far the most likely cause is a too unrestricted desire for food on the part of the man. Here also in both of these cases it appears to me that the thought is before the thing, not the thing before the thought, that it is the desire which, being gratified, causes the irregularity, the excess, or the defect of the function, pointing to the conclusion to which the study of life in all its aspects seems to lead, that the government of desire and a wise self-restraint make things go well, and safely, and properly. So then if on the one hand we see the organism healthy and active in the pursuit of good, which is to be shared with all its fellows, we may feel assured that there has been a wise government of desire, and if on the other we see commerce

pursuing a course, not monotonous, indeed, but varying within certain well marked limits, neither marked by boom nor by panic; we may also infer on the part of the business community a wise and just self-restraint, a well ordered thought, which has translated itself into that suitable and proper thing.

CHAPTER VII.

Some Further Observations on the Two Great Medical Paradoxes as related to Changes in Function.

AS health and disease shade off into one another by insensible gradations, it is often difficult to say where the one ends or where the other begins. Would it, for instance, be a just or normal use of words to say that a man whose sleep function was disturbed, was diseased, and if so, at what point in sleeplessness should we say that disease set in? If a man does not sleep for, say, one night or for two nights, are we justified in saying of him he is diseased? Want of ease, dis-ease in this sense, he is suffering from; but I think it will be admitted that to speak of such a man as being diseased would scarcely be to use the words *disease* or *diseased* in their ordinary sense. Where, then, is the difference between health and disease to be found? Suppose, for instance, that in place of the function of sleep, we were discussing the function of circulation, or of respiration, or of intestinal action. If we define the function of circulation in health as corresponding with a pulse rate varying

between, say, 60 and 90 beats a minute, and if we find a man with a pulse rate of 55 on the one hand, or one with a pulse rate of 95 on the other, are we justified in saying, on this evidence alone, that either of them is diseased? Scarcely, I think. Although a healthy pulse rate is generally found to coincide with a rate of not less than 60, or not more than 90 a minute, still it is a difficulty that, when dealing with organic affairs or the phenomena of organised bodies, and especially when dealing with the phenomena of organised bodies so complicated as the human economy, we cannot offer definitions which are true to a nicety. Health shades off into disease by insensible gradations, and although, as a rule, a healthy pulse rate is generally found to be running as stated, and a healthy respiratory rate between, say, 13 and 18 a minute, and although a healthy temperature is generally about 98·4° F. (37° C.), still we should not be justified, I think, in saying—at least I think it would scarcely accord with the ordinary usage of words, if we were to say that a pulse rate of 55 on the one hand, or 95 on the other, or a respiration rate of 12 or of 20 a minute, or a temperature of 97·5° F. or of 99° F. were necessarily marks or proofs of disease. I should at least scarcely feel justified in speaking or writing in this sense, although I daresay I should feel less difficulty in admitting that a condition of disease were present if I observed a pulse rate as low as 40 on the one hand, or as high as 120 on the

other. Where, then, it may be asked, is the difference between functional alteration and organic disease? This is rather a difficult question, although no more difficult than many others which call for answer in other domains of knowledge. No child would confound a dog with a gooseberry bush, and yet it might tax the wit and information of the keenest and best stored intellect to offer such definitions as would separate logically and in characters all plants from all animals. The fact is that animals and plants shade off into one another by gradations so insensible that it is sometimes very difficult, and sometimes even impossible, in classifying a given specimen, to be sure to which class or kingdom we ought to refer it. This kind of difficulty, indeed, abounds in our study of nature. Even between things so immensely apart as living and dead, or organic and inorganic, how difficult it is to state the differentia. Or between gaseous and fluid, or between fluid and solid. *Practically,* of course, these questions do not trouble us, or not very often; but *academically* it must be admitted that they present very great difficulties indeed. Between sane and insane again the experience of our law courts, as well as judgments of sensible and unbiassed persons, shew how difficult it is to draw a dividing line. Insane people do not necessarily do insane things; in fact they comparatively seldom do so; but they do sane things (if, indeed, one may speak of things or actions as being sane or insane?) in an insane way.

But who shall say certainly whether a given action was that of a sane or of an insane person? Disproportion is probably the most unfailing mark of insanity, the failure to make ideas, and the actions springing from them, to correspond with the realities of things; and therefore persons are said to be sane or insane much rather because of the ways in which they do things, than because of the things themselves which they do. In fact the things done by insane persons are very often things which require to be done by sane persons, but insane persons do them in excess, either greatly too much and too vehemently, or greatly too little and too casually, or they may fail to do important things at all. But to decide whether a given action was that of a sane or insane person, is often so difficult that one expert may think it was the action of a sane person, and another may think it was not. Similarly with the intestinal function, which we expect to act once a day. We should hardly say if it missed a day on the one hand, or acted twice a day on the other, that a man was diseased; although if there were inaction for a fortnight, or five or six motions in a day, we might be more disposed to admit that he was so. The difficulty is that both of these opposite conditions may be present without the person thinking himself ill enough to consult a doctor, although he did not in either case think that he was quite well. Perhaps, however, persons look with more equanimity or with less mental disturbance on the former state of

inaction than they are disposed to do on the latter state of over-activity.

These cases are instances of the well known difficulty of drawing the line, a difficulty which meets us in so many cases in life that each of us at once thinks of many instances in which he has experienced it. And in the present case, although we cannot absolutely separate (can we absolutely determine anything?) between alteration of function and disease, we may make an approximation to the statement of the differentia; and the test that I think may be offered, practically, is that in disease there is generally present some exudation from the blood, or from the lymph, in some part or parts of the body, while in merely functional disturbance there is not. I quite admit that there are cases, well recognised to be cases of disease, where no such exudation can be shewn to be present, a mild feverish attack, for instance, or a mild attack of typhus fever, or of measles. But as shewing how functional change and disease shade off into one another, we have only to picture a very severe case of typhus fever in which exudations take place into the arm pits and groins, and result in suppurations such as are sometimes seen in plague. From this point of view, sleeplessness would not be a mark of disease, although it would be a mark of functional disturbance, unless we could shew, or unless it were likely, that some exudation from the blood into the parts of the nervous system, through which sleep is produced, or into its connective tissue,

were present to account for it. So with constipation, or with diarrhœa; and so with disturbance of circulation, or of temperature, or of respiration, or of any other function of the body. It may be argued that no change of function occurs in the body without some corresponding temporary change, say of circulation or of nervous action, leading insensibly to change of structure. That may be so. But so long as we cannot shew, or render likely by reasoning the presence of some exudation from the blood, somewhere in the body, as accounting for the disturbance of these functions, we use the term disturbed function, or functional disturbance, rather than disease. If, on the other hand, we know or think that we know that there is anywhere in the body some exudation to account for the disturbances, then we speak of disease. I believe myself that a large number of instances of functional disturbance commence in changes, and generally in some blockage of the lymph circulation in the capillary vessels lying between veins and arteries. Very soon, and often impalpably or unprovably, some lymph congestion of connective tissue sets in, some phase of what I have called *Initis, and that this is the beginning of the process of disease in a very large proportion of cases. But, of course, if anyone were to argue that, before these changes occurred, some previous mal-assimilation was present, some failure to assimilate nutriment, this in turn being caused by unsuitability in food supply as

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* See Chapter VIII. for a full account of Initis.

regards its quantity, quality, or frequency, this statement would also accord with my view. Practically, I should say that disease generally or almost always begins at some point in the digestive, and blood making, and lymph circulation processes, that is, that disease is a form of mal-nutrition. And I have a general opinion formed from the evidence offered throughout this essay, that the particular phase of mal-nutrition which is commoner than all the rest put together, or at least the most common, is the mal-nutrition which comes from excess of food. Then the most usual change to which this excess of food gives rise is over-growth of the connective tissue; so that, if we had functional change in the connective tissue of a nerve, we should speak of neuralgia (*νεύρον* = nerve, and *ἄλγος* = pain), while if we had exudation present, or thought we had, either in the nerve or in its sheath, we should speak of *neuritis* or *peri-neuritis*. The former would be functional disturbance, the latter, disease. Mere sleeplessness would be functional disturbance, but *cerebral meningitis* or *cerebritis* itself would be disease. Palpitation, that is, irregular action of the heart, would be functional disturbance, so long as there was no organic change in the organ; but *endo-carditis*, or *peri-carditis*, or *carditis* itself would be disease. And so with mere constipation and mere diarrhœa as contra-distinguished from *enteritis* or *peri-enteritis* (*peri-typhlitis*, *e.g.*, which is a rather more familiar term). Perhaps I ought to explain here, for the

information of the general reader, the meaning of the termination *itis*, appearing in the words *neuritis*, *cerebritis*, *enteritis*, *bronchitis*, &c. The termination *itis* is the end of a Greek feminine adjective, meaning *of or belonging to*, and agreeing with *νόσος* or *νοῦσος* = disease, understood. Thus *cerebritis* would be a diseased condition of or belonging to the cerebrum or brain; *enteritis* would be a diseased condition of or belonging to the *ἔντερον* or bowel; *bronchitis* would be a diseased condition of or belonging to the bronchial mucous membrane, &c., &c., &c. The termination would always point to a diseased condition of the part of the body named in the beginning of the word. But as it was soon seen that the commonest diseased condition of parts was inflammation of the parts, the termination *itis* has for a long time now, by general consent, come to mean inflammation of the part pointed to by the beginning of the name. Thus *cerebritis* means inflammation of the cerebrum or brain, *enteritis* means inflammation of the *enteron* or bowel, &c. In all these conditions there is an exudation temporarily effused into the parts, and so in the generality of cases the differentia that separates disease from mere functional disturbance is found to be present. With one word more I may for the present dismiss this discussion. The bodily organs, and even the tissues, very frequently consist of three parts. (1) The tissue or organ proper, its interior part, so to call it, or what is sometimes termed its *parenchyma*. (2) Its coverings, or

sheaths, or envelopes, These coverings are generally two, the inner and the outer. The Greek term for a covering being εἴλεμμα, inflammation of the εἴλεμμα becomes *eilemmatitis* or *ilemmatitis*, and hence we say diseases are *parenchymatous*, on the one hand, when the intimate central structure of the organs is inflamed, and they are *eilemmatous*, or *ilemmatous*, on the other, when the coverings are inflamed. As either the inner or the outer covering may be inflamed, we sometimes speak of *end-ilemmatous* (ἔνδον = within) and *ex-ilemmatous* (ἐκ or ἐξ = without). An *end-ilemmatous* disease would be represented by *bronchitis* or inflammation of the inner lining membrane of the lungs, while *pleuritis* or *pleurisy* would be the name used of inflammation of their outer or serous membrane. Sometimes the inflammation of the outer covering is referred to by the Greek preposition περὶ = around, and hence, instead of *ex-enteritis* or *exo-typhilitis*, we speak of *peri-enteritis* and *peri-typhilitis*, meaning inflammation of the parts around the τυΦλὸν or blind portion of the bowel (the Latin *caecum*), so named because it ends in a *cul de sac*. So much it seemed necessary to say in order to clear up some points in medical terminology, which seem sometimes to create confusion in the minds of readers.

We can now go on to discuss a little more at length the alterations of function, which appear according as more or less stimulus is acting on the body. From time to time it will be evident

that function shades off into disease by insensible gradations, so that it is often difficult to differentiate the one from the other. But the propositions I am about to make are true, it seems to me, of function rather than of disease. The general proposition or propositions are :—(1) In the absence of stimulus there is no function; (2) in the presence of too much stimulus there is also no function; and (3) when stimulus is moderate and proportionate function is normal and healthy. We saw that these propositions appear to be true for the functions of intestinal action and of sleep. We have now to see that they can be generalised, when they will be found to be true of all functions. Of course, to do this completely would involve a considerable amount of labour, since to prove it completely would be to evolve a complete treatise on physiology, on the one hand, and a complete account of the practice of medicine on the other. But short of this too great task, it is possible for us to get much probable evidence of the truth of this generalisation. And *the* stimulus, which, as in the case of sleep and intestinal function, has an influence so great as to cast all others into the shade, is the stimulus of food supply. Other forms of stimulus are important, no doubt, but this is the chief, and that by a long way. It is not necessary for us to depreciate the effects of air on the body, or of work, or of anxiety. In some ways, indeed, the effects of air-supply on the economy are more noticeable than are the effects of food-supply; for deprivation

of air for even a few minutes brings life to an end; while the body can go on without food (if, that is, it is meanwhile supplied with water) for an indefinite number of weeks, according to the resistance of the organism, but certainly in many cases for not less than six, while some persons have lived without food for eight or ten weeks and even longer. I suppose this is one of the reasons why in our sanitary discussions, and in our practical measures founded on our sanitary ideas, so much attention has been given to the effects of air supply, and so little to the effects of food. I do not know for how long an average person, not exposed, say, to extremes of weather, can live without both food and water. For how long, for instance, could an average adult of 35 years of age, and in average health, live, say, in a covered boat gently rocked on a calm and temperate sea, if deprived of food and water? Perhaps two or three weeks? But if supplied with water and deprived of solid food, he can live for six or eight or more weeks. But if deprived of air (and this, however he had been fed before), for not longer than a few minutes. There is a very great and very marked difference. The effects of work or muscular movements, again, or of anxiety, are, like the effects of food, of an indefinite length of duration, so that the same difficulties present themselves to us in the attempt to guage the effects of these as in that to shew the effects of food. There is a great difference then between the view here stated, that food supply, the defect of it,

the excess of it, the irregularity of it, and the good or bad qualities of it, is far the most important cause of alteration of function in the body (leading to disease, no doubt, since function and disease shade off into one another by insensible gradations) —there is a great difference between this view and that which should state that food supply is the only predisposing cause of disease in the body. Anyone, indeed, who should attempt to argue the latter view, would put himself out of court at once, rendering himself unworthy to obtain a hearing. My argument, then, in attempting to shew that food supply is the chief cause of illness, must take the form of shewing that when defect of function exists, on the one hand, when excess of it exists, on the other, or when irregularity of it is present, these abnormalities can be made to disappear by alterations in the diet better than by any other means. To do this I propose to attempt to deal with other functions besides those of sleep and intestinal action, with which I have already dealt. Let us consider (1) circulation, (2) nutrition, as shewn in obesity and in attenuation, (3) respiration, (4) renal action, (5) the heat function of the body, or the maintenance of its temperature, and (6) cerebration, or thinking, feeling, &c. In dealing with all these (as also with others, which, however, are not quite suitable for general discussion), it will be found that the paradoxes formerly stated hold good as practical conclusions, and that we may say :—

1. Circulation. Too great slowness of the pulse, say a pulse of 40 a minute or even 20 a minute; too great quickness of the pulse, say up to 130 or 140 a minute; and irregularity of the pulse, all may and often do depend on an excess of food, and all can be combated, therefore, by restriction of the diet. Otherwise, we can quicken a slow pulse or render regular an irregular one (that is, one which is now too slow and then too quick), by the same means by which we can slow a quick pulse, namely, by restricting the diet.

2. Nutrition. Too great thinness and too great stoutness, and the state in which persons sometimes become too stout, quickly, and, again, too thin, quickly (in which nutrition is irregular), all may and often do depend on excess of food; and, therefore, we can often fatten too thin persons and render much more regular the nutrition of persons who thin and fatten alternately, at too short intervals of time—we can regulate these abnormalities by the same means by which we reduce or thin obese persons, namely, by restricting the diet.

3. Respiration. Too great slowness of respiration (below twelve a minute, *e.g.*), too great quickness of respiration (above twenty a minute, *e.g.*), and irregularity of respiration, that is, the state in which too great slowness at some times alternates with too great rapidity at other times, may and often do depend on an excess of food. And, therefore, we can frequently quicken a too

slow respiration, and regulate an irregular one, by the same means universally adopted to slow a too quick respiration, namely, by restricting the diet.

4. Renal action. Too scanty and loaded urine, as well as too thin and too abundant urination, as well as irregularity of the function, when too scanty urination alternates too rapidly with too abundant functioning, all may and often do depend on an excess of food. From which it follows that we can frequently increase a too scanty urination, and regulate an irregular flow, by the same means by which we can reduce a too abundant flow (as, *e.g.,* in what is termed diabetes insipidus or even diabetes mellitus), viz., by restricting the diet.

5. Temperature of the body. Lowness of temperature in the body (a temperature of 96° F., or less, *e.g.*), elevation of temperature in the body (a temperature of 102° or 103° F., *e.g.*), and irregularity of temperature in the body, that is, the state in which a too low temperature alternates at too short intervals of time with a too high temperature — these three states of disturbed temperature all may and often do depend on an excess of food. From which it follows that we can frequently elevate a too low temperature, or regulate an irregular one, by the same means which are almost universally adopted to reduce a too high one, viz., by restricting the diet.

6. Dulness of cerebration, or mentalisation, or thinking, when the person's mind is lethargic

and sluggish, when, *e.g.*, he tends to fall asleep if he tries to think or even to read; also too great rapidity and want of continuity in thinking, when he cannot keep his mind on one subject for any length of time, but flies off suddenly to another; as also the condition of mind in which sluggish thinking alternates too rapidly with excited and perhaps flighty thinking—these three states of disturbed mental function may and often do all depend on an excess of food. From which it follows that we can frequently quicken the thinking powers in too great sluggishness, regulate them in too rapid alternation of quickness and slowness of thinking, and slow them when the processes are too quick—we can often effect all these changes by restricting the diet.

If the critic were to say, as perhaps he will say, that all these changes in the various functions of the body may be brought about because persons are exposed to the influences of bad air, or because persons are deprived of proper exercises, or because they are exposed to too great anxiety, or because persons have too many things to think about, or because of the incidence of other causes, we ought to admit all these as possible causes, or we might theoretically admit them to be possible causes. But practically I have no doubt at all that the influences of any two of these causes put together, or even of all of them, are not to be named in the same breath as the influences of improper food supply. Further, of the three or four possibilities of food supply,

its excess, its defect, its irregularity, or its bad quality, I have no doubt at all that excess of supply, either as to quantity or frequency, or both, is far the commonest way in which this cause acts. So that the practical conclusion we form whenever we see alteration of function, either as to excess or defect or irregularity, is this: In order to rectify the excess, the defect or the irregularity, let us first restrict the diet, and see what the result will be. I might have added the function of the skin to those already named, and might have stated as to the action of this seventh function: Too great dryness of the skin, when, for instance, the hands feel hot and dry; too great moisture, as when they are wet, cold and clammy; and the state in which too great dryness alternates at too short intervals of time with too great moisture, all may and often do depend on excess of food. From which it follows that we can frequently relieve too great dryness of the skin, as also too great moisture, as also irregularity in the manifestation of these conditions, by restricting the diet. In reference to the skin function, I will only say that more than once has it occurred to me to be able to regulate it by the simple advice to persons suffering in any of the ways named, that they should, *e.g.*, omit their supper.

Summing up then the propositions regarding the relation of food supply as a stimulus to changes in function, we come to the following statements:—

1. In the absence of the stimulus of food there is no function. (If organs are not supplied with nutriment they cease to act).

2. In the presence of too much stimulus there may also be absence of function. (If organs are supplied with too much nutriment they cease to act.)

3. In the presence of moderate stimulus there is normal function. (If organs are supplied with their proper amount of nutriment, neither too much nor too little, function is normal or natural). These are very different propositions, I may say, from the statement that might have been made that, for example, absence of function is *caused* by absence of stimulus. I do not think we are entitled to say the latter. What we are entitled to say is that absence of function and absence of stimulus are co-ordinated and co-related. We are not entitled to say which is first—which is the cause of the other—at least, not till after a long philosophical inquiry, very interesting, no doubt, to some minds, perhaps to all, but to enter on which, as it would lead us very far on the present occasion. there is no time nor opportunity for. Let me here just remind the reader of what was before said as to the various relations of thought and thing; and ask him to apply the same ideas to the relation of food supply and function. Food supply would correspond with the thing; function with the thought. The same general relations hold between food supply and function as between thing and thought.

The reader who sees the statement that excess of food supply is a far commoner cause of altered function than either defect, or irregularity of supply, or even than bad quality of it, may find himself in a difficulty. If he should whisper to himself—what about poverty, then?—do the very poor also eat too much and too often? It seems to me that they do. The poor, it seems to me, eat poor food too often and too much, and the rich eat rich food too often and too much, and they are both ill. *Frequent* eating is nearly always of too much bread, cakes, and sweets. I never met any person, man or woman, who ate meat five times a day, but I have met very many persons, especially women, who took bread, not only five times a day, but even six, seven and eight times; and once a woman came under my care who ate bread no less than ten times a day. I got to know all these people because they were ill—they suffered in a variety of ways, from colds, headaches, &c.; but a way in which many of them suffered was in being so thin, so wasted, and so attenuated, that some of them were advised that they were not having enough food. A lady who ate eight times a day, on going to a watering place, was told: "Madam, you are low, you are weak, you are attenuated, and you must be fed up." The advice, I suppose, was felt to suit her complaint, her mental if not her bodily complaint, and she got, as so many people do, the advice which it was felt would suit her. A servant in a common lodging house, who got up at

4-30 in the morning, and began with her tea and bread and butter then, keeping on at it all day till I counted ten times that she was taking it (the ordinary dinner about 1-0 p.m. was one of the ten meals), she, poor thing, was advised to go to bed on nothing but milk and soup diet for a fortnight, after which her diet was cautiously increased, until she was got on to proper food habits ; and she did very well. *She* also was low, weak, and attenuated, and she was fattened by having her diet for a time greatly restricted. Her condensed, and plugged, and obstipated tissues were drawn on, to deliver up some of their too solid constituents, those whose too close aggregation made her too thin ; by this means the blood found its way better to the tissues, and she improved in colour and aspect under restriction, and by and bye increased in weight. But although, as I say, bread, and cakes, and sweets are the commonest foods which are taken too often and too much, and although I have never met anyone who took meat even as often as five times a day, not to speak of six, seven, eight or ten times, still, no doubt, if anyone did so foolish a thing, we should find that he suffered from some form of serious illness. The *principle* is that too much food and too frequent consumption of it is destructive to health ; the *accident* that it is nearly always bread, cakes, and sweets, rather than meat, eggs, or game, which are taken in this unphysiological and unhealthy way. I have no experience of the effects of the

appalling poverty which are said to be experienced in the east end of London. I do not know how much of these effects is due to crime, vice, recklessness, drink, and improvidence; but, judging from experience in other places, it seems as if very careful elimination of these sources of sickness would have to be made before the effects pure and simple of insufficient feeding in causing sickness and mortality could be declared with any approximation to accuracy. In provincial England, at any rate, whatever may be true of the capital, even the very poor, as a rule, eat too often and too much; and the charwoman is made ill and has her life shortened, not by insufficiency of diet, nor yet by the hard work which she is always talking about, but by the five meals which she thinks it necessary to take in order to keep up her strength so as to be able to do her work. Even the beggar's baby suffers in the same way, and from the same causes, viz., not too little food too seldom administered, but from taking it too often and too much. For the beggar's baby, which has bread and tea at eight in the morning, before being taken out to beg, which gets from the hand of charity a piece of bread and butter at 10-30 in the morning, which is taken home at 12-30 or so to dinner of bread and tea, and which is again fed with the same food at four and at seven before being put to bed, suffers also from a succession of "colds," and often from one or more of the "fevers," such as measles, scarlatina, diphtheria, infantile remittent fever, or the like.

These ailments are attributed to cold, exposure, infection, etc., and it seems to occur to but few that the chief predisposing cause of them is improper feeding. Nevertheless, the sickly history of the rich woman's child, in whose case cold is avoided and exposure and infection are carefully guarded against, shows that there is a common cause of the illnesses both of the children of the poor and of the rich, viz., improper feeding, the commonest form which this takes being, not too little food administered too seldom, but too much food administered too often. Of course, occasionally, direct starvation is a cause of mortality among infants, but the instances in which, *e.g.,* infants are neglected by bad mothers are nothing like so numerous as are those in which they suffer from too much food injudiciously administered too often. The latter cause of death often takes the form of what Dr. King Chambers termed the starvation of over-repletion, or, as we might term it, of indirect starvation—the former is direct starvation. Both indirect and direct starvation cause or may cause thinness, wasting and attenuation, but obviously thinness caused by indirect starvation must be treated by restriction of the diet, while thinness due to direct starvation must be treated by cautious increase of it. The mortality among infants is frightful in England and other countries. How frightful may be judged when we reflect that among the whole population of England and Wales about a fourth of the children die before they are

five years of age. But, as many careful and well-advised mothers rear all their children, at how great a rate must the others die, to render the general mortality so high!

Mr. Seebohm Rowntree's interesting book on the Poverty of York appears to me to point in the same direction. His menus of the meals of the poor shew bread and jam four times a day, and sometimes five times a day as the staple of the kinds and frequency of their food supply. This is certainly in accord with what strikes me as the true conclusion in the matter on the whole (there are exceptions, no doubt), when I say that the poor eat poor food too often and too much, and that it is a common cause of their illnesses. Two meals a day, one of which might well enough be the bread and jam referred to (it is quite suitable for one of the daily meals), and the same amount of money spent on more substantial fare such as frumity with suet dumplings, and some egg, or milk, or meat up to the value of the three or four bread and jam meals which the second meal replaces, would provide a much healthier and more substantial dietary, and would cost no more, would indeed, I think, cost less; so that the needs of the family whose total earnings are twenty-five shillings a week or less, would be met, as far, that is, as it is possible to meet them on such small means.

In attempting now to make good the statements that both excess and defect of function, as also irregularity of function, are frequently due to

excess of food, and that these conditions can be more easily treated by restriction of the diet than in any other way, it is not necessary to say much regarding the treatment of excess of function, for this is almost invariably and by almost universal consent treated in this way. A too quick pulse, too rapid respiration, excessive renal action, a too high temperature, obesity, and even excessive mentalisation, as shewn, *e.g.,* in delirium or too great talkativeness, are usually treated by restriction of the diet. There may perhaps be exceptions in the case of excess of renal action, and occasionally in that of too great mentalisation or cerebration, but the almost invariable custom regarding the others is to restrict the diet. As regards excessive renal action, such as we get in diabetes, when persons pass an immense quantity of urine, amounting perhaps to 30 or 40 pints in a day, the best results can often be obtained in this way. In such cases I have seen the quantity brought down in a few weeks from many pints to three or four in a day, and with a great fall in the specific gravity, by the administration of milk and barley water, with mutton tea, during the day, and nothing (except water) during the night. One such case was that of a young woman, twenty-three years of age, who was brought to me by her mother, suffering from diabetes (specific gravity of urine 1040), many pints of urine containing large quantities of glucose being daily passed. There was also some albuminuria. Under a diet of half a pint of milk and barley-

water, equal parts, twice a day, and half a pint of mutton tea in the middle of the day, continued for six weeks, this young woman entirely recovered, after which she was put upon two meals a day, the advice being given her to take a little meat at one of her meals (about four ounces), and some bread at the other. Under this regime she remained well, and then, feeling that she need not keep so strictly to orders as before, returned to her usual three meals a day. In a year from the time of her being first seen she relapsed, the old diabetic condition returning, and being removed by the same curative regime as before. The second attack did not last so long as the first one.

It will be observed that the theory on which treatment in this case was conducted was that the disease was due to too much food taken too often, and not that it was due to too much starch and sugar in the diet. The advice given, therefore, was that the patient should diminish her food or restrict her diet, not that she should abstain from starch and sugar. I have reason to believe that, had she been fed on milk, meat and eggs—which the theory that her illness depended on too much starch and sugar would have necessitated—she would not have recovered. It is interesting that a difference of view so slight as that a patient's condition depends on too much food rather than on too much starch and sugar, should make so much difference to the prospects of recovery, that on one view she would recover, and on the other not. An academic

difference of opinion as to causation may translate itself into the difference between recovery and non-recovery in the sphere of treatment, or into a difference, so far as the patient is concerned, between life and death.

A man, thirty-eight years of age, who had suffered for two years from diabetes mellitus, was put upon a diet of a cup of coffee for breakfast, a basin of soup for dinner, and a glass of milk and barley-water for supper ; and this treatment being maintained for some weeks, the specific gravity of the urine fell from 1040 to 1020, the glucose diminished greatly, and the quantity passed was much lessened. After that he was advised to take one meal daily of ordinary mixed diet, with milk for his evening meal, sometimes with an onion. Under this diet he improved greatly, and lived for several years, dying, however, in the end, of the remote effects of the intemperate habits to which he was formerly addicted.

In another similar case, a man improved greatly for some weeks on a similar diet, and seemed to be in a fair way to recover, when unfortunately the love of alcoholic drink to which he had formerly been addicted, overcame him, and giving way to alcoholic intemperance he speedily relapsed and died.

On the other hand, a middle-aged man suffering from diabetes mellitus at the same time as the two last-mentioned persons, and who was fed, on medical advice, on a diet of milk, meat and eggs, died in six months of diabetic coma. The view taken of

the causation of his illness was that it was due to too much starch and sugar ; and the excess of food, which was most probably the real cause, was continued in a somewhat modified form, an excess of mixed diet being replaced by an excess of nitrogenous food, and the patient lost his life. Here again a discussion, which sounds an academic one only, between the merits of too much food, on the one hand, and too much starch and sugar, on the other, or between the general and the particular, may, when translated into treatment, be fraught with the issues of life or death to a patient.

In other cases I have seen great benefit from the administration of half a pint of milk at 8 a.m. and 4 and 8 p.m., with 4 ozs. of beef or mutton and some green vegetables at 12 noon. This is of course a greatly restricted diet, whether as measured by its calorie value or by custom. Professor Davis, whose work on the Diet Treatment of Disease has already been referred to, speaks of "a reasonable restriction of the diet" in the condition in which too much water is being passed.

Cases of too great mentalisation or cerebration or of too rapid, and irregular, and flighty thinking, very often find their way to asylums, and are therefore not so much treated by doctors outside of these establishments ; but the forced and sometimes mechanical feeding which is often resorted to there is of very doubtful value, and seems frequently to be over-done. When patients in these places refuse food, as they so often do, the proper plan, it seems

to me, would be to wait until they come round, and to refrain from interference until signs of danger to life set in, which would generally be not after a few days, but after the lapse of some weeks. I was present on one occasion at a discussion of this question, at the meeting of a medical society, at which several asylum experts were also present, and only one case could be cited in which a person in an asylum had carried starvation to actual death. All the rest had given it up after some time, and had returned more or less to food. Dr. Dewey, of Meadville, Pennsylvania, publishes a remarkable case in which a young lady in such circumstances was induced to leave an asylum and to put herself under the care of a scientific man who believed, in accordance with Dr. Dewey's teaching, that too much food is often the chief cause of insanity. Under his advice she began to fast, neither he nor she knowing for how long the experiment was to be allowed to continue. He meant to interfere if he saw any danger of death from direct starvation. He believed she was insane from indirect starvation, otherwise from the effects of too much food taken too often, which had so altered her blood as to supply the brain badly, and so to cause the general flightiness which compelled her friends to send her to an asylum. The well-meant but ill-judged plan of treatment there was to feed her five times a day. After fasting for 45 days, during which time she took, besides water, no food at all, she became perfectly sane, and has since remained so, having

returned, of course, to proper but not excessive feeding, meantime. It may also be mentioned that from the 24th to the 45th day of the fast she walked over 100 miles. Her portrait and name are published by Dr. Dewey. On looking at it, one would, I think, say she was "neurotic," but neurotic people may be sane or insane according to the ways in which their environment alters, and one of the chief causes why such persons not infrequently pass the border line between sanity and insanity is, I believe, through the mismanagement of their food. I feel quite sure that many persons, especially women, are in asylums mainly through this cause, and that much of the flightiness of conduct and hysteria as it is called, manifested by many women in conducting the business of their lives, and that of their households, and who are not in asylums, is due to the same cause. I have had a most striking proof of this related to me in a long letter, the writer saying, after her recovery, that all the while that she was behaving so strangely as to become at last quite intolerable to her friends, she knew what was going on and *knew the cause also* (for she had been told what it was, viz., an inordinate liking for sweets), but for a long time she could not brace herself up to do without them. When at last she did so, however, and restrained her abnormal appetite, she again became sane and quite her former amiable and reasonable self.

I do not think that the very potent influences of causes of this prosaic kind in disturbing mental

equilibrium, especially in women, has received anything like adequate recognition. If it were to receive it, much more could be often done by very simple means to restore patients to mental health, and prevent the need of their being removed to asylums. If this is so, we can see how insanity is often brought on just as other diseases are, just in the same ways often as an ordinary cold or sore throat is induced, and how ridiculous it often is to suppose that heredity, *e.g.*, has any more influence on the disturbance of mental balance than it has in inducing many other ailments. Further references to this point of view will be made later. For the present, the important point is to notice that patients shewing signs of flightiness, and loquacity, and oddness in ordinary life outside of asylums, can often be well treated by a milk diet, and frequently do so well on it as not to require removal. But a diet of two pints or three pints of milk a day is a greatly restricted diet. In calories it does not amount at the highest estimate to more than 600 or 900 against the 2303 calories emitted by the starving man. Even six pints of milk a day—and to see how much this amounts to (a glass, *e.g.*, at 4, 6, 8, and 10 a.m., 12 noon and 2, 4, 6, 8, and 10 p.m., 12 midnight, and 2 a.m.), let anyone try to keep up the ordeal of taking this quantity for a week, and he will find out what an intolerable burden it is. But even so, six pints of milk a day only amounts to 1800 calories, and the starving man emits 2303 ! ! I knew a woman suffering from

typhoid fever who died from no other cause than stuffing her with milk at this rate. If she had had two and a half or three pints a day she would probably have recovered. I need not, I think, dwell further on the management of excess of function. It is only another name in most cases for the feverish state, and it is invariably and by general consent, and quite properly, treated both by medical practitioners and by the public, by restriction of diet. It is true that illustrations of excess of function, especially great excess, have to be obtained from what is usually termed disease, but this only shews, what is freely admitted, that healthy function shades off into disease by insensible gradations. The practice of medicine is only applied physiology and applied pathology, but between physiology and pathology there are only the differences that are found between function and disease, viz., differences of degree. The differences are those which obtain, *e.g.*, between day and night, not differences of kind, or quality, or essence, but of degree only—not differences between contradictories, but only of contraries.

I am afraid I shall find much more difficulty, and shall encounter much more opposition in the attempt to establish the proposition that defect of function must also, in the vast majority of cases that come before us, be treated by restriction and not by increase of the diet. Nevertheless, the references already made will help us to understand this part of treatment better. Of course I do not

deny—let me repeat this—that defect of function is occasionally due to direct starvation. In this case it must be treated by cautious increase of food. But this is a comparatively rare occurrence in ordinary practice, so rare that it may practically be put out of account. The great majority of cases of defect of function are, on the other hand, caused by excess of food (or of some other stimulus—but excess of food is by far the commonest form of over-stimulus), and must therefore be treated by restriction. Let me deal with the various functions seriatim, and in the same order as before. First as to circulation. I saw a boy of ten years of age, not long ago, whose pulse rate was 40 a minute, and the pulse was also irregular (there was defect of function and also irregularity of function). He got quite well, that is, his pulse rose to 80 and became quite regular on a glass of milk and barley water three times a day, or rather, on half a glass of the mixture given six times a day, and nothing but water in the night. This treatment was continued for a fortnight, when his pulse having risen to 80 and having become regular, he was given one meal in the middle of the day, and the milk and barley water continued to the exent of half a pint of the mixture morning and evening. After about ten days of this treatment, and when the circulation had become well re-established he was given an ordinary meal at tea-time, and his mother was recommended to continue this diet for the boy after he went back to school, that is, he was to have two

meals a day, dinner and tea, and a glass of cold milk and boiling barley-water, equal parts, in the morning before he went out. Similar treatment, but continued for three or four weeks in the first stage, namely, half a glass of milk and barley-water, equal parts, every two hours in the day, and nothing but water at night, raised the pulse of a man from 38 to 72 a minute. After this he was treated as the boy was, by cautious increase of the diet, that is, he was put first on one meal a day, and after a couple of weeks on two daily meals, and became quite well. It will help to avoid repetition if I say here, that in both of these cases I recommended the continuance of two meals a day, and of two only, after recovery – what may be called a dissiteous regime – on the following grounds. Too much food taken too often appeared to be the chief cause of the illness in both cases. It was necessary to greatly restrict the diet in order to obtain or to allow recovery, and this restriction had to be carried to a point below what would have been sufficient for the maintenance of life and vigour, especially as all work was perforce stopped for the time being. But if, after recovery, the patients had been advised to return to a diet as full as that on which they had previously fallen ill, it is plain that they might be expected to fall ill again, either in the same ways as before or in some other way. In order, therefore, to avoid this evil, they were both recommended in future to pursue a dissiteous regime in place of the three or four meal plan

(the trissiteous or even tetrasiteous plan) formerly pursued, and the results have been very satisfactory. When defect of function (in these two cases, too great slowness of the pulse) shews itself from food taken too often or too much, we may say that there is present in the body irritation with tolerance, the irritation shewing its primary effect, viz., depression or shrinking. On the other hand, when a quick pulse is caused by excess of food supply, we may say that there is present irritation with intolerance, the irritation shewing its secondary effect, viz., reaction or swelling. These two states may be illustrated by the behaviour of disciplined soldiers in attempting to get away from danger, as compared with the behaviour of an ungoverned panic-stricken crowd. When a thousand soldiers attempt to go too quickly through a pass which will allow only six at a time to pass, they may become blocked and slowly come to a standstill. But the state of irritation with intolerance is like a crowd of persons flying from a burning theatre, jostling one another, blocking up the doorways, and trampling on one another in wild panic. If the former offer a rather helpless mark for the attack of an enemy, the latter not only do this but are their own enemy, and are self-destructive besides. In both cases, however, the remedial indication is to diminish the numbers attempting to pass, and to diminish the causes that led to the block ; but there is greater need for haste to act in the latter condition than in the former ; and the analogue of the way in which the

numbers attempting to pass may most certainly and most safely be diminished in both conditions of the body is by restricting the supplies of food which led to their multiplication or increase in the first instance. If these ideas are kept in mind we easily see how both contrary states must be treated in the same way. Neither the boy nor the man had alcoholic stimulants administered. They might have had, but they did not. The essence of the treatment was the restriction of the diet. There is a long, low depressed state of the circulation which follows influenza sometimes, in which the pulse becomes very slow and sometimes irregular. This also ought to be treated by the administration of hot fluids and by restriction of the diet in the way suggested. When it is not, but is treated by increase of the diet, as it too often is, the patient sometimes succumbs to the treatment, while his best if not his only chance would have been from the opposite method of treatment. How serious is this difference of view and of treatment between different medical experts may be imagined. I feel convinced that it forms a great part of the reason for the low esteem in which the medical profession is frequently held by the public, a low estimation, notwithstanding the laudatory things they say of us when in other moods, too often justified—for if the experts are not only not agreed, but are actually in flagrant conflict, what confidence can be placed in them? Imagine a business expert called in to advise the community how to mitigate the

stagnation of business caused by a plethora of stocks. If in such a case he should advise increased manufacture so as to multiply the stocks, his clients would feel that he had misapprehended the problem, even if they could not quite see how. But if another expert advised using up the existing stocks before proceeding to manufacture new ones, a long suffering public could only feel that both experts could not be right, and might be driven to conclude that no confidence could be placed in either, or in the expert class as a whole. And yet one of these experts would be right, and the other wrong, so that the last conclusion might be very unjust.

2. The function of Nutrition. The next function to be considered, if we follow the former order, will be that of nutrition, in the restricted sense in which it may be said to cause obesity and attenuation. (Of course, in a wider sense, nutrition may be said to cover all the functions, and changes, and metabolisms of the body). There will be no difference of view that obesity must always (or at least almost always?) be treated by restriction of food and drink. But attenuation, or too great thinness, and emaciation—is this always or is it generally to be treated by increase of food? Let us see. The young woman formerly mentioned came to me suffering from indigestion, anæmia, and emaciation. She had been ill for three years, and, for the last two years before I saw her, had been unable to follow her very light occupation of a

paste-board box maker. (This occupation consists mostly in sitting with small pieces of paste-board in the hands, and allowing a triangular-shaped iron, driven by power, to come down and make "corners" on the paste-board. It is rather a monotonous occupation, but is in no sense laborious, so that the person who could not do that must have been weak indeed. If ever any person required to be "fed up," surely it would be a person in such a state, and, indeed, this was exactly what the eight doctors she had seen before had advised her—but she was not better, but worse). For the first four weeks after she came to see me, she took a tumblerful of milk mixed with an equal quantity of boiling water, morning and evening, and about half a pint of mutton - or chicken - soup in the middle of the day. I need not go further into particulars about this case than to say that under this greatly restricted diet the young woman gained $1\frac{1}{2}$ lb. in weight in three weeks. She was suffering from the "starvation of over-repletion," and under this restricted diet she gained weight. At the very most this diet does not represent more than 450 calories (probably 300 calories is nearer its energy value), while the starving man emits energy value up to 2300 calories. She was, therefore, under a starvation diet, and under it she gained weight ! ! How? Because her tissues were too dense ; they were obstipated or constipated (so was the action of her bowels, which slowly rectified themselves under the treatment), and the blood, which was not deficient

at all, but in excess, could not flow to the tissues, and so could not nourish them properly, and she became pale and thin, and wasted and weak. When, under a starvation diet, her body was induced to some extent to live on itself, the tissues slowly began to be less dense, the blood flowed into them better and nourished them better, and, as she probably replaced some of her too dense tissues with water, which was not restricted, though other forms of food were, she gained weight. There was no difference among the experts as to the facts. The girl *was* anæmic, she *did* suffer from indigestion, she *was* pale, and thin, and wasted. But there was all the difference in the world between the experts as to the interpretation of the facts, and there was also the greatest difference in the results of treatment founded on the different and opposing views held by the experts. This kind of case can be parallelled every day in practice. Another illustration of increase of weight following restriction of the diet will be found in the case detailed in the next chapter, where a child of four years of age gained 9 lbs. in weight, on a diet described by physiologists as a starvation diet, continued, not as in the case referred to, for a short time, but for the long period of fifteen months. What evidence could be more striking or more convincing to the candid inquirer than this sort of evidence? Truly they who do not yield assent to it will not be convinced by anything.

3. Respiration. There is a phase of the

respiration in which the breathing becomes very slow, with long pauses in between. Instead of breathing fourteen or fifteen times a minute, a person will breathe only eight or even six times a minute, three or four breaths taking place quickly after one another, after which a long pause occurs, lasting in some cases for half a minute, to be followed by three or four more quick breaths, and then another half-minute pause. The breathing is not only slow, but irregular, though it is often, as will be seen, regular in its irregularity. In this condition the state of the patient is generally very serious—in fact, very often this condition precedes death—so that in general nothing which we can do is of any avail to lift him out of his state of too slow breathing. Nevertheless, under the administration of hot drinks, whose aggregate calorie value in twenty-four hours does not amount to more than 400 or even less, this slowness and irregularity of breathing sometimes quickens, and becomes more regular, and patients survive for a time, till another attack comes on. There will probably be no difference of opinion as to the treatment to be adopted for such a condition. No doctor that I have ever heard of would advise the administration of much food or of solid food in such a condition, though he might term the frequent administration of small quantities of fluid "keeping up or nourishing" his patient. It sounds better to the ears of the friends to term it so. But scientifically, so far as the calorie value of such a

diet is concerned, it is a restrictive diet, and not a "nourishing" one. Therefore I conclude that depression of the respiratory function is treated by common consent, and ought to be treated in the vast majority of cases by restriction, and not by increase of the diet. Of course, if slowing and irregularity of the respiration had occurred among Mark Twain's men exposed in the boat, and if it had, therefore, been due to direct starvation, as it might have been, it would have been necessary to treat it by cautious increase of food—but this is the uncommon or exceptional case, while the other is the common one. It is hardly necessary to say that too great rapidity of the respiration is by universal consent and quite properly treated by restriction of the diet. It is in fact one of the most striking marks of the feverish state, which is always treated by restriction.

4. Renal Action. Defect of this function or scanty urination usually depends on over-nutrition, and ought, therefore, to be treated by restriction of the diet. I think I need not dwell much on this view because it is a generally accepted canon of treatment that scanty urination, as it is usually a mark of the feverish state, must be treated by restriction of the diet. But it is a curious thing, shewing how far, as yet, medical treatment is from being, as it ought to be, a set of examples of applied physiology, that opinions as to treatment still differ, not as to defect of the renal function, but as to its excess. It is, as we have seen, quite the

reverse in the case of the three functions already considered. In these there are differences of view as to how deficiency ought to be treated, but none as to excess. If we think about it, however, we shall see that a scanty, high coloured, and loaded urine indicates a plugged or blocked renal action, corresponding with the ordinary form of constipation of the bowels, while diabetes, with or without the presence of sugar (diabetes means free urination, from διὰ and *βαίνω*, = *I pass through*), indicates the excessive renal action which corresponds with long continued diarrhœa. As constipation and diarrhœa are both generally caused by excess of food, and as they must both, therefore, be generally treated by restriction of the diet, we can the more readily see how the same canons of treatment should govern the management of the renal function in opposite conditions, and we can the more easily understand the rationale as well as the efficacy of the treatment of the cases of diabetes already detailed in this chapter.

5. The Bodily Temperature. A low temperature ought also to be generally treated by restriction of the diet. No doubt in direct starvation the temperature falls, and where a low bodily temperature of say 95° or 96° F. is caused in this way, it must be treated, if we have any opportunity of treating it at all, by cautious increase of the diet. But the commonest cause of subnormal temperature is not deficiency of food, but excess of it ; and equally obviously this must be treated by restriction of the

diet. A great difficulty in accepting this view, a difficulty so great that it seems to render the defence of the view absurd to common sense, is that persons suffering in this way are often thin, emaciated, and attenuated by their illness, and it seems ridiculous to advise restriction. In fact, persons no doubt often become very angry, and change their medical adviser, if he advises restriction in this condition. And if they do not, their friends do it for them. To meet this difficulty, the medical adviser is often compelled to call his method by the name of frequent and nourishing feeding, and in order to seem to justify this, he orders small quantities of hot stimulating fluid food (not necessarily alcoholic stimulants), at short intervals of time. A tablespoonful of milk and hot barley water every half hour, or the same quantity or even double the quantity of the same, or of hot beef tea, or bovril, or some such food, taken all day long, sounds as if it were a large quantity of nourishing food. In point of fact it is not. It is a very restricted diet. Suppose that a patient takes an ounce of milk and hot barley water every half hour from say 8 a.m. till 10 p.m. How much does she take in a day? Two ounces an hour for 14 hours, or 28 ounces in a day. Even on the assumption that the barley water is as valuable a nutrient as the milk, the total calorie value of such a diet is not above 450 calories (it is probably not so much as that), and we have seen that the starving man emits a value of 2300 calories. But people are so

easily led by words that they dismiss the medical adviser who speaks of restriction—which such a diet really is—and send for him who calls the same thing a "highly nourishing diet." In point of fact I raised the temperature of a man who was, besides, thin, emaciated, and attenuated by constant vomiting, lasting for seven years, from 96° F. to 98·4°, by advising him to fast for 35 days. On the 28th day his temperature had risen to normal, and remained so. I do not think it could have been raised in any other way. Neither does he. *He* also was very angry when the suggestion was made to him; it seemed so much opposed to reason and common sense; and it was only when he felt that he was dying on the other plan of frequent feeding, and when he reflected that he might as well die on one plan as on another, that he consented to submit to treatment. I believe myself that he *was* dying on the other plan, and that the method of treatment that saved him was the only one that would have done it. Or at least I believe—what alone I am concerned to make out—that restriction of the diet was indicated in the treatment of the case. How much restriction was necessary, and whether absolute starvation, except for the free use of water, for five weeks was necessary, must be left to the opinion of the medical adviser. But the thin, emaciated man, though he lost some 13 lbs. more weight during the fast, not only did not die of it, but recovered; and during the time of the treatment his bodily

temperature rose over 2° F. (I believe it rose 3° F., but say over 2° F., because I wish to keep within the bounds of fact). Could anything have demonstrated more clearly what the cause of the man's illness was, and what the cause of his subnormal temperature was, than his behaviour under the treatment? In seven years a man has many opportunities of knowing his condition and of hearing the views of different advisers. And his unwillingness to submit to treatment makes him all the more valuable a witness to its efficacy, after he has submitted.

6. Defective Cerebration. This condition also, as usually met with, is due not to too little food but to too much, and generally requires restriction of the diet. Of course, here also the cause may be long continued and direct starvation. In this unusual case it must be treated by cautious increase of the diet. The history of the case alone will determine which is the cause, but in ordinary practice the cause is so seldom deficiency of food that it may practically be left out of account. I have already said something about this condition at pp. 246-7, and must refer the reader to what is there said, for my defence of the opinion that, the cause being generally too frequent feeding and its consequences, the condition calls for restriction of the diet. I have seen some very notable cases in which this was true, and in which also the opposite line of treatment was very damaging to the patient. This kind of case, however, as it leads so often to

deprivation of liberty, and stirs up, besides, fierce passion because of the horror, the altogether foolish horror which persons have of having their friends confined in asylums because of their prejudices regarding heredity, cannot be so fully entered into or described as some others. If it were generally realised that brain irritation—both the irritation with reaction or intolerance, as I have called it, and the brain irritation with tolerance, which shews itself in depression—is very often caused in just the same ways as the taking of any other illness, then we might be allowed to talk more calmly and dispassionately about it. When dealing with the subject of the heredity of disease, and when attempting to distinguish between the organisation with its qualities, which *is* generally transmitted, and acquired disease itself, which is generally *not* transmitted, I hope to return to this point. Meantime, I think it will be well to close the general discussion, as also to refrain from considering some other functional changes which may occur to the imagination of the reader, but which are unsuitable for public consideration. One point I should mention about these mental conditions, viz., this: both excess of cerebration and defect of it (as well as irregularity) are not infrequently caused by mental anxiety and stress; while wrong feeding, and especially too frequent feeding frequently acts as a contributory cause. Still it has to be admitted that long continued anxiety alone may upset the mental balance. In fact,

however a man or woman lives in respect of his diet, he may be over-borne, and his mental balance upset, by anxieties of business and of domestic and social circumstances too great to withstand. Anxiety is no exception to the general rule that moderation is the only fixed and the only unfailing rule; but, although without the stimulus of moderate anxiety, we may tend to become lethargic and unwilling for exertion, still under too much pressure we may lose our balance altogether, and drop into melancholic depression on the one hand, or react into maniacal excitement on the other.

I think, however, as the result of this long discussion, that I have been able to shew the two paradoxes in action, viz., first, the paradox that the same causes induce opposite states in the body, and, second, that opposite causes induce the same state. And I think I have also shewn, or, at least, have rendered it very probable, that of all causes food is the most important, so that in finding ourselves called on to treat excess of function, defect of function, or irregularity of function, we should eliminate the effects of food in causing the condition before we proceed further in search of causes. Our view as to causation may or may not affect our treatment of cases (although it generally does, whether we know it or not), but the prevention of diseases is so wholly dependent on our ideas as to causation, that it is absolutely necessary for us to make up our minds on it if we wish to prevent them.

CHAPTER VIII.

The Quantity and Quality of Food Required by Man. The Number of Meals, and the Intervals of Time that Ought to Elapse between Them.

I HAVE already said that the function of food is to make blood, and that it is only by ellipsis that the function of food can be described as being to nourish the body, to supply energy to the body, to maintain its structure and tissues, and to provide for their repair. It does all these things, no doubt, but indirectly, by supplying material to form blood; and it is directly the function of the blood to do all these things, and only indirectly the function of the food to do them. This being so, the question before us in this chapter is how much food must be daily taken into the body of an average man or woman, in order that the blood may be so made, and enriched, and refreshed, as to suffice to nourish the body properly, and so as to supply the material whose oxidation or combustion may be convertible into energy or work done, and to maintain the bodily heat; as also that proper maintenance and repair of the tissues may be effected. An additional question has also incidentally arisen through discussions in previous chapters,

and we want to know also how food and blood may do all these things without letting the bodily functions be checked by too little on the one hand, or clogged on the other by too much; since we have seen that in both of these states the bodily functions are impaired and rendered inefficient, and that, if either the checking or the clogging processes are pushed, the bodily functions are either stimulated to great excess of action, or tend to come to a stand-still. A great deal of discussion has always taken place around this question of food-supply to the body; and there are commonplaces in inference and in observation which have naturally occurred to inquirers in all times, as well to ancient writers on the subject as to the modern ones, who, since the time of Liebig, have considered the chemical composition of food, and who, since the time of Count Rumford and Joule, have been put on the track of considering the mechanical equivalent of heat, and its conversion, by mechanical and animal machines, into physical and physiological work.

I have already at pages 140-2 mentioned three important observations made by the ancient Greek medical authors, which have a bearing on this subject, and need not repeat them here. But it would obviate a good deal of needless labour if persons who have proposals to make on this and cognate subjects would familiarise themselves with what has already been said and done in these matters. Even the distinguished physician, who

has re-introduced the no breakfast plan as a practical proposal for the circumstances of to-day, introduces his ideas in the following words:—"A hygiene that claims to be new, and of the greatest practicability, and certainly revolutionary in its application, would seem to require something of its origin and development, to excite the interest of the intelligent reader." But, as we have seen, the advice is not new and revolutionary. So far from being either of these, indeed, it does not go so far as the ancient writer in its recommendation, since Dr. Dewey does not recommend the taking of less than two daily meals generally, while the advice of Hippocrates to go *ἀνάριστος* was offered to persons who were taking *ἄριστον* and *δεῖπνον* only, and therefore amounted to a recommendation that they should be monositeous and eat once a day only. There would or might be less objection to Dr. Dewey's proposals on the part of critics if they had taken the trouble to inquire what no less an authority than the father of Greek medicine himself had said on the subject, and if they realised that it is only a very ancient advice resuscitated, and that very much is to be said for it, which it would be well for them to consider.

The reader will long before now have gathered, I hope, that the writer of this essay is of opinion that humanity suffers far more from being clogged by too much food than it does from being checked by too little. If there are a few people still in this country who suffer from direct starvation, there

are vastly larger numbers, he believes, who suffer from the starvation of over-repletion. It is not the poverty of England at the present time which is doing it harm; it is its wealth. Perhaps this is not quite a fair statement; and we ought rather to say, since neither wealth nor poverty does us harm in itself, that it is the use we make of either of them which affects us well or ill. The present writer at least believes that if the effects of over-feeding were eliminated from medical practice, only a small remnant of diseases would be left. What remnant? Perhaps a quarter or a third at the outside; perhaps a tenth on the least calculation. That is to say, he has come to the conclusion that if people generally would reform their diet—which means in general terms, if they would consent to reduce their diet, for he does not think that it is this, that, or the other article of food that is doing damage, but it is eating (and drinking) too often and too much—if they would reduce their diet, young, middle-aged, and old alike, the illnesses that affect humanity would fall by from nine-tenths to three-quarters or two-thirds as compared with those from which humanity now suffers. There would not be more than a third or a fourth, perhaps not even more than a tenth part of them left to trouble us. The remarks made about the two great medical paradoxes have, he hopes, somewhat confirmed this view, and have enabled the reader to understand how the attenuation due to the clogging or choking by too much food can easily be mistaken, and often

is mistaken, even by the experts, for the checking which is due to too little. In other words, he hopes he has somewhat cleared up the fallacy of confusing the effects of *direct starvation* (from too little food too seldom administered), with those of *indirect starvation* (from too much food taken too often).

With these conclusions in our minds, let us now shortly view the present position of the question of food supply to the body. An immense amount of work, by very well informed and very capable men, has been put into the question. Two defects, however, it seems to the writer, characterise the whole of this work. First: in order to determine the quantity of food required for the nourishment of the human body, the quantity of the egesta from kidneys, bowels, lungs, and skin, has been computed over a large number of instances. It has been found that about twenty grams of nitrogen, and about 300 grams of carbon are eliminated daily by the body of the average man; and hence it has been computed that these amounts must be supplied daily in the food administered, since nothing is more certain (there can indeed be no dispute about this point) than that the body can create nothing, and that, if it is not supplied with materials to make good its losses, it must waste and consume away before its time. But what if the average man is in the habit of consuming more food than is good for him? Will not the egesta in this case be larger than if he

takes only a proper amount of nutriment? Of course they will. And if from the egesta of too much food habitually taken, we infer the proper amount of food required to keep a man in health, shall we not be apt to prescribe more for him than he requires, and so, by our prescription, choke the functions of his body with too much, and so lower his vitality by this means? Evidently we shall. What should we think of the engineer or the stoker who should gauge the quantity of coal required by his engine by the quantity of ash left after combustion? Plainly the quantity of ash left is directly proportional to the quantity of fuel put into the furnace, and also inversely as the effectiveness of the combustion, for every stoker knows (even if he has not put his knowledge into words), that he can easily put his fire out by heaping on too much fuel. No; the proper inquiries for the engineer are to ask how much heat he requires to provide for the performance of a certain amount of mechanical work, how much heat he can get out of the complete oxidation of a certain weighed quantity of fuel, and therefore how much fuel he requires to consume before his engine can do the amount of mechanical work (or of heating) required of it. The questions asked by the animal physiologist and by the doctors are precisely analogous; and if they gauge the amount and frequency of administration of food by the quantities of carbon and of nitrogen eliminated by the body, they will be very apt to

fall into fallacies of reply just as would the engineer. Now this is exactly what has happened in this enquiry. When we look at the arguments of the present physiological authorities on this subject, the force of this fallacy becomes apparent.

Another fallacy incidental to this question has been that the inquiry how much food is required by the body has not been made over a long enough time, and in particular that no sufficient enquiry has been made whether the man remained well or became ill under the administration of food. It is true that in prisons and among soldiers and sailors some enquiry has been made into this question, but hardly any has been made into it as regards the general population. But to refrain from doing this is really to fall into a very great fallacy. A man or woman may undoubtedly take for a day or two, or for a week or two, or longer, a quantity of food quite unsuitable to health, either in amount or quality, without seeming to suffer much in one way or another; and yet, if he goes on doing so, may suffer from illness from the continuance of its effects. It seems to me, I must say, that the frequent illnesses from which humanity suffers offer a *prima facie* case for the suggestion that the way in which we live is somehow unsuitable to us; and as by far the most important means by which the blood and tissues are modified is through the food consumed, the suspicion seems to arise in our minds that all may not be well with the ways in which the body is managed in regard to this. When we come

to inquire further, and in greater detail, as to this, our suspicions become confirmed, and the answer seems to arise in by no means inaudible tones that our food habits will require much modification if we are to be or to remain well. "We habitually eat more," says Professor Nathan S. Davis, of Chicago, "than is needed to supply waste." All authorities more or less agree in this opinion. But what the writer thinks has not been sufficiently realised is that habitually to eat more than is needed to supply waste, translates itself and must translate itself in the conduct of life, into suffering from a large variety of illnesses, and that until this cause of illness has been eliminated we must continue to suffer in these ways, whatever our doctors do for us, and however expert they may become in finding remedies for the relief of ailments brought on by this cause. Another consideration brought out by this line of reflection is that a constant succession of illnesses means early senility and early death, even when patients recover from illness after illness, since it cannot be denied that the body is weakened by a succession of illnesses; and that although Nature is very skilful in eliminating waste effete materials from the blood and tissues by the recurrence of the illnesses which she inflicts on the body, still not all of the *materies morbi* or *materies morborum* is eliminated each time, some remaining to clog and choke up the processes of the body, so as to bring it into the heaviness and ineffectiveness of old age, and even to death, much before its time. And as,

after recovery from each attack, the former methods of living are again resorted to, and as, under the plea that a larger supply of nutriment is necessary to restore the strength lowered by the illness (to some extent a true and just plea, but apt to be greatly overdone), even more nutriment is administered than before, the train of causes is efficiently laid for the onset of another attack, which occurs accordingly, being followed in turn by another attack, and still another. It is in this way, and for this reason, that, for example, we hear of persons having a long succession, one after the other, of severe colds, or of their having six or eight or ten, or even occasionally fifteen or sixteen, or even more, attacks of influenza, this condition of constantly recurring illness not being confined even to the laity, but occasionally affecting medical practitioners themselves. We have already seen, even although it is lengthening a little, how unsatisfactorily short life is for the large majority of the human race living under the ordinary conditions of civilisation in a settled country like England. A return of the Manchester Unity of Oddfellows, recently made by Mr. Alfred W. Watson, one of the actuaries of the society, has brought out the general facts that the duration of life is increasing among the members of the society, but that it is accompanied by a larger amount of sickness than formerly. The return covers the years 1893-7, and comprises a much larger number of years of life exposed to risk, and also a much larger number of sickness-weeks than has been

dealt with before. The general results seem to be these: There has been an increased sickness among members aged 17-45 years, as compared with 1866-70, of 21 per cent.; between the ages of 45 to 65 years there is an excess of sickness equal to 26 per cent.; and from the age of 65 and over there is an increase of 42 per cent. As to mortality there has been an improvement as compared with the results of thirty years ago. Up to 45 years of age the death-rate now is only two-thirds of what it was thirty years ago. This is what our general examination of the subject had already led us to expect. From 45 to 65 years the death-rate has been found to be nine-tenths of what it formerly was. At ages beyond this we find a heavier death-rate. Whatever the causes of these general results are, it does not seem to be very satisfactory to contemplate an improvement in the duration of the life of working people up to 65 years of age, when it is accompanied by a considerable increase in sickness. More life and more efficient is what we want, not more life and less efficient. Over 65 years of age, among this large class of workpeople, both the mortality and the sickness rates are larger. The improvement in early life is due, I suppose, to the diminution in the fevers and in consumption which have occurred in the thirty years ending in 1897, these being, as we have seen, diseases whose incidence mainly falls in the earlier years of life. The benefit has been gained, as we have seen, by the attention which has been paid to the improve-

ments effected between the body and air, and the improvement holds on to a period well past middle life. What the subsequent deterioration is caused by is not apparent. Of course if persons live longer than formerly, they must die at somewhat increased rates at the later ages, since they cannot go on living indefinitely. But whether this accounts for the whole of the increase, I do not know. Evidently, however, the increased mortality at the later ages cannot be caused by either increased hard work or increased liability to accident, since the increased use of machinery has tended to displace laborious hand-work by the lighter work of superintending the machines which do the work; while the efforts of the legislature to prevent accidents have been so far successful that there is at least no increase of mortality or of sickness on this score. Whether the deterioration of health at the higher ages is due to the increased number of meals which working people, as well as other people, are now taking, is at least a question worthy of consideration.

Let us now hear what different physiological and other authorities say regarding the requirements of the body as to food supply.

With a body weighing 74 kilogrammes, or about 164 pounds, Ranke found himself sufficiently nourished with—

Proteids	100 grammes.
Fats	100 ,,
Carbo-hydrates	240 ,,

This he could have got from—

	Proteids. Grams.	Fat. Grams.	Carbohydrates. Grams.
Lean meat, 9 ounces, or 250 grammes, containing	55	8·5	0
Bread, 18 ounces, or 500 grammes, containing	40	7·5	245
Butter, 2 ounces, or 55 grammes, containing	0	55·0	0
Fat, 1 oz., or 28 grammes, containing	0	28	0
Total	95	99	245

or, since 18 ounces is altogether too much bread for a man to eat in a day, say, nine ounces of bread and one pound of rice pudding, containing one egg, and say, six or seven ounces of lean meat. This is the allowance for a large man. Most men weigh a good deal less than this; but even so it is about 30 ounces of mixed diet daily. At the same rate, a man weighing 120 pounds (we are not told if Ranke's weight included his clothes) or three-quarters of this weight would require three-quarters of this allowance of food daily. That is, $6\frac{3}{4}$ ounces of lean meat, about 13 ounces of bread, $1\frac{1}{2}$ ounce of butter, and $\frac{3}{4}$ ounce of fat—*i.e.*, about 22 ounces of food as it comes to the table.

But we are not told for how long Ranke persisted in taking this diet, and whether, for instance, he had any "colds," rheumatism, bronchitis, headaches, or other ailments. Most townsmen of my acquaintance would suffer from dyspepsia, constipation, *herpes labiorum*, bronchitis, rheumatism, *pruritus ani*—one or more of these

affections—if they took anything like so large a quantity of bread as 18 ounces daily for any length of time. Nine ounces of meat is also a large quantity to take daily.

The demands of Moleschott are higher than those of Ranke, viz.: Proteids 120 grammes, fat 90 grammes, and carbo-hydrates 333 grammes. But most men habitually take much more food than these allowances.

It has become customary in recent years to estimate diets in what is called calorie value, and perhaps we ought to do so here. Food eaten represents so much potential energy, capable in the body, as fuel in a furnace, of being converted into so much kinetic energy, which latter takes the form of maintaining bodily heat, or of doing work. "To determine," says Professor Davis, "the amount of potential energy in food eaten, it is necessary to know how much food is consumed and the potential energy of food stuffs. The calorie is the unit that has been fixed upon to express the energy stored in food. A calorie is the amount of heat required to raise one gram of water 1° C. The most convenient way in which to estimate the value of food stuffs is to determine how many calories a given weight will furnish. It has been established that one gram of dry proteid will furnish 4000 calories, the same quantity of carbohydrate 4180 calories, and the same quantity of fat 9400 calories. With these facts known, it is easy to determine the calories that any given food stuff will furnish,

provided the percentages of proteid, fat and carbo-hydrate that it contains are known. These percentages must be multiplied by the figures just given, and their results added, in order to ascertain the number of calories in 100 grams of food." This is the scientific doctrine of the day, what may be called the formal statement of the scientific position on the subject. Obviously it requires translation into pounds or ounces of ordinary mixed food as it comes to the table, and proportionment to the business of life, whether that of brain-worker or hand-worker, and if so, whether a light hand-worker or a heavy hand-worker, before it is of much use in practical life to practical people. Ranke's diet aforesaid obtainable from the meat, bread, butter, &c., mentioned, works out to about 2355 grand calories. (A grand calorie is 1000 small calories, or the quantity of heat required to raise 1000 grammes of water through one degree Centigrade. I always state results in grand calories, and to avoid the use of long figures; for 2335 grand calories would be 2,335,000 small calories, a longer and probably a more confusing number). Moleschott's diet works out to 2718 grand calories. Playfair's full health diet of 118 grams proteid, 50 grams fat, and 528 grams carbo-hydrate, comes to 3140 grand calories. The same authority makes the diet of active labourers demand 155 grams proteid, 73 grams fat, and 568 grams carbo-hydrate, which comes to 3630 grand calories. I append below a short table embodying

the views of other authorities, so that the reader may see at a glance how widely they differ on this subject.

Authority.	Proteids.	Fats.	Carbo-hydrates.	Calories
Voit—Moderate Work ...	118	55	500	3055
Do. Hard Work ...	145	100	450	3370
Atwater —Little Exercise ...	91	91	300	2450
Do. Light Muscular Work	100	100	350	2800
Do. Moderate ,, ,,	127	127	500	3520
Do. Active ,, ,,	150	150	500	4060
Do. Hard ,, ,,	177	250	650	5700

It appears to me—I respectfully suggest it to these eminent authorities—that all these dietary requirements are too high as measured by the test as to whether the persons who take them remain well for a long time, say a year or two, or longer. It seems to me that all my experience says, and that firmly and decidedly, that such diets make people ill, at least those people who live in towns and do town work. They cannot bear them without suffering in a variety of ways. A patient of mine, for example, suffering from a variety of ailments, was advised to greatly restrict his diet in order to get rid of them. He belonged to that large and increasing class of the community living in towns, who now form the bulk of the population of these islands, and who may fairly be described as persons with little muscular exercise or as doing light muscular work. He suffered from dyspepsia and heartburn, frequent colds, resulting in bronchitis and recurrent attacks of sneezing and bronchial asthma, recurring attacks of herpes of the lips,

that is, eruptions of watery blisters on the lips, which in course of time became dry and scabbed over and then cleared off; recurring little sores similarly produced on the tongue, constipation, and general rheumatism, making his back and limbs and muscles ache and become tired long before the reasonable and moderate exertion he made could possibly account for it; in fact, he often woke tired in the morning after six or seven hours sleep; and sometimes he could not sleep till say three o'clock in the morning, while sometimes he would sleep for two hours, and then waking might have an attack of asthma which would keep him awake not less than two hours, while occasionally he could not sleep at all. For this large variety of ailments, plainly portending the more or less speedy onset of more serious and possibly incurable disease, he was advised greatly to restrict his diet. He weighed about 140 pounds, so that according to Ranke he required 85 grams proteids, 85 grams fats, and 205 grams carbo-hydrates. What he did take in two meals, seven or eight hours apart from one another, was the following:—Lean meat, 3 ozs., or fish, 3 ozs.; bread, about 4 ozs.; rice, about 4 ozs., uncooked (4 ozs. of uncooked rice equals 1 lb. of rice pudding); butter, about 2 ozs.; cheese, 1 oz. to 2 ozs. and over; and one egg on some days, and some days no egg. This works out to about 60 grams proteids, 85 grams fats, and say 150 grams carbo-hydrates. After losing weight a little on this diet, he afterwards regained it up to about 133 lbs.,

and incidentally he got rid of every ailment he had, so that from being a delicate, pale, pasty-looking man, disinclined for exertion, he became well coloured, healthy-looking, and fresh and able for exertion at all times of the day. I do not think that Ranke's requirements are far out for the average townsman doing office work or the work of an active business man, or the work of a townsman generally, be he professional or layman, but I think even his diet errs on the side of too much rather than on that of too little. It will be observed that my patient's diet works out to about 1666 grand calories, *i.e.*, 60 grams proteids × 4, 85 grams of fats × 9·4, and 150 grams carbo-hydrates × 4·18, and this is just about two-thirds of what Ranke's diet amounts to (2325). But Ranke's diet is lower than Moleschott's, or than Voit's, for moderate work, or even than Atwater's for little exercise, while it is very much less indeed than Atwater's diet for active work (4069 calories), or for hard work (5700 calories). I confess that it is a severe violation of my conclusion from all the observations that have forced themselves on me in a prolonged experience to be asked to believe that almost any man can stand for long a diet amounting to 5700 grand calories daily. However strong a man might be who took this amount of food, and however hard he worked, I do not think he could possibly oxidise it off, and I feel sure that between overloading his digestion and overworking his muscles he would speedily fall into an incurable illness on such a

regime. Other authorities mention enormous quantities of food as justifiable to be taken in health, as, *e.g.*, $4\frac{3}{4}$ pounds of mixed diet, while even Dr. King Chambers, who introduced the phrase "Starvation from over-repletion,"* recommends the nursing mother to take three pounds of mixed diet daily. In my opinion, the ordinary townsman requires $1\frac{1}{2}$ pounds of mixed diet daily, from that as a liberal estimate down to three-quarters of a pound as a not unusual one, if he wishes to be well and to keep so, while a woman requires even less than this. This would be my translation into the language of every day life of Professor Davis's statement that "we habitually eat more than is needed to supply waste." We cannot go on putting into the animal economy more supplies than are needed for its smooth and easy and efficient working, without rendering the working of it hard, difficult and inefficient by clogging and choking it up.

Contrast with these diets Dr. Haig's Diet Table II., in his little book, Diet and Food in Relation to Strength and Power of Endurance. Table II. is as follows :—5 ozs. Hovis bread, 2 ozs. oatmeal, 1 oz. gluten, 3 ozs. cheese ; or a total of 11 ounces of solid food with one pint of milk daily. If the two ounces of oatmeal be made into half a pound of porridge, the total weight of this diet is

*Note.—My friend Dr. Dewey has used the phrase "starvation from over-feeding," not knowing of Dr. King Chambers' prior expression. Here again we are forced to exclaim: How one generation discovers, how its successors forget or overlook, and how we have to discover again, to the great loss of our time and damage to progress!

17 ounces and a pint of milk; and Dr. Haig says it is enough for the average man. I think he is right; but if this is so, it at once follows that all of us take too much food, for certainly none of the people of my acquaintance take so little food as this (except, indeed, some of those who have been very ill, and have reduced their diet under medical advice).

Cornaro, it is well known, lived from the age of 38 years, when he suffered greatly from dyspepsia and many ailments, till the age of 97, or for a period of 59 years, on a diet of 12 ounces of mixed food daily and 14 ounces of red wine. His age at death is variously given, but was not less than 97, and he himself believed it might have been much more if he had not abused his constitution in his youth.

In connection with Cornaro and the length of time he lived, and that in good health, on a very restricted diet, I cannot forbear making an observation. It was Addison who introduced the story of Cornaro to English readers. It was he who translated from the Italian the treatises which Cornaro wrote on health and longevity; and it was Addison who knew, if anyone did, how longevity had been attained. How did Addison behave himself? After breakfasting about eight or nine in the morning, he would drop into a coffee tavern perhaps in the forenoon, sipping coffee and stronger drinks. He would lunch about one o'clock, and spend the afternoon as he spent the morning; and

dine (or sup) from seven to eight in the evening, keeping up hilarious nights till the small hours of the morning. The consequences of this were, as Addison must have well known that they would be, death at 49 years of age, from severe asthmatical distress of breathing, and general dropsy.

In infirmaries, where, if anywhere, persons ought to be properly fed, it is customary to allow about 16 ounces of bread daily, besides other food, to each patient, This is very much too large an amount for health on the average, Anything beyond from six to eight ounces of bread daily is sufficient to cause in men and women doing ordinary town work a variety of ailments, most of which are usually attributed to "taking cold." Common examples of these are watery blisters on the lips and tongue, pimples and spots on the face, so frequently seen in young people who are fond of bread and cakes, and take them too often and too much, inflammation of the windpipe and bronchial tubes, inflammation of the mucous membranes of the eyes and nose, sore throats, enlargements of the glands of the neck, constipation (or diarrhœa sometimes), and rheumatism, &c. Although the remote effects of the digestion and assimilation of food is to increase the strength and to build up the tissues of the body, it does not seem to be generally realised as it ought to be, that the *immediate* effect is to place a labour and often a severe labour on the body, and that this for a time shews itself in depression of the body by using up part of its

energy in the digestive processes. Too frequent repetition of this labour in the body by the taking of too many meals, especially when the amounts of food taken are greater than the bodily requirements, often seems to exert an influence of cumulative depression, and accounts for the subnormal state of the temperature, which so often precedes the onset of chronic and of incurable disease, as was shewn in the chapters dealing with the paradoxes of medicine. And it was then also shewn how a hot drink (of coffee, *e.g.*), might raise the bodily temperature for an hour or two, although not itself supplying material for oxidation and combustion. The cup of afternoon tea often acts in the same way, and its mode of action explains also the advice given not to eat anything with it, since food interpolated then will find lunch undigested, while in turn it will not itself be digested when the evening meal is taken. It cannot be wise to have digestion going on at two different stages in the same stomach at the same time. And if the temperature of the body is subnormal from an excess of material present there is all the more reason to abstain from food, which, if taken, can only aggravate the depression already existing, after the immediate effects of the stimulation shall have passed. And study of what goes on in such circumstances in the body explains how fasting may sometimes be of such great benefit, since fasting, by inducing the organism to use up some of the material accumulated in it in excess, takes

the load off to some extent and enables the organism to rise to natural, when the burden clogging it in every capillary vessel and in every lymph duct has been lightened. A case which I treated some years ago illustrates so well the points just referred to, and also raises for our consideration another method of estimating the quantity of food required by the body, that, although I have already mentioned on p. 261 another feature in the case, I may be pardoned for recapitulating the main facts of it here. A man 47 years of age, a labourer in a foundry, when he was able to work, but who had not worked for over seven years at the time of which I speak, submitted on my recommendation to a long fast in order that he might, if possible, be cured of recurrent vomiting, which had troubled him for all that time. He had had much and careful treatment from distinguished medical men, but with little or no benefit. Except that he had the whey from two pints of milk daily, he had no food of any sort except water from 31st August till 5th October, 1898. The solids in 30 ounces of whey do not at the outside amount to more than two ounces (I do not myself think they amount to so much), but we shall assume that the man had two ounces of solid food daily during that time. Now what was the weight loss of his body? It amounted to 13¼ pounds in 35 days, not lost continuously and equably during that time, but intermittently. From the ninth to the thirteenth

day of the fast, for instance, he lost no weight at all, while from the twenty-ninth to the thirty-fifth day of the fast he lost three pounds in weight. But on the average the loss during the 35 days was about six ounces. This, added to two ounces of food taken (if it was so much), implies that the man's economy used about half a pound of the material of his body daily on the average to keep himself alive; and, therefore, I take it we are entitled to conclude that about eight ounces daily of mixed diet is sufficient to enable a man of 130 pounds weight to subsist and keep himself alive if he does not do any more work than my patient did, that is, walk about, and help his wife in little domestic duties like making beds, laying the table, and washing up dishes. This loss of eight ounces daily corresponds well with some other observations of mine, and cannot as a subsistence diet, be considered unusual. For instance, a lady patient of mine, suffering from rheumatism, constipation, and a variety of ailments, maintained her weight for a considerable period of time on eight ounces of food a day, her health improving very much in almost every particular during the process, including the complete cure of constipation. When she added two raw apples daily to this diet, as a second meal, she slightly gained weight. From facts like these I conclude that eight ounces of mixed food daily (not, of course, eight ounces of white bread, but of mixed diet) is a subsistence diet for people moving about and doing no work beyond

the lightest sorts, or such work, for instance, as a sempstress does. They can just maintain their weight on it. It does not seem, therefore, an unreasonable inference from these facts to conclude that persons could do the active but not laborious work of an overlooker in a mill, of a weaver, of a joiner, of a doctor, of a lawyer, or of a shopman or clerk, or of a lady looking after the conduct of a household, or, in fact, of the average town-dweller, on an amount of food represented by the addition of say six or eight ounces more. This 14 or 16 ounces of food brings us very near to Cornaro's 12 ounces, with 14 ounces of red wine, rendering his estimate and practice a far more reasonable and credible amount than we might have thought if we had had regard only to the almost invariable habit of mankind to take much more. It contrasts strongly with the light or moderate diet of Ranke, which would amount with the water of cooking added to not less than 30 ounces. But when we come to think of $4\frac{3}{4}$ pounds or four pounds of food, it does not seem possible that in any conceivable circumstances so much can be required ; and if so much is taken, it is difficult to escape the conclusion that the body must suffer for it in a variety of ways. Next, let us consider the late Dr. King Chambers' advice that the nursing mother should have three pounds of ordinary food daily, and whose only fear was, as he said, lest she should not have enough. A baby will require from 12 ounces of breast milk up to say two pints daily, according

to age. This would involve the addition to the mother's ordinary diet of say four ounces of extra food. Or suppose that she took even two pints of milk daily over and above her normal food, that would only be 600 calories. Even if her ordinary diet consisted of Ranke's demand for a large man, she could not possibly be made out to require more than two pounds of food, so that Dr. King Chambers' demands for three pounds is one which, if satisfied, must clog the woman's economy and make her ill, even if she added to her nursing labours that of wheeling the perambulator, as many mothers do. But if her ordinary diet consisted of say 16 ounces of food, then her nursing diet might amount to 20 ounces, or $1\frac{1}{4}$ pound, or perhaps, if we are very liberal, to $1\frac{1}{2}$ pound, just half of the amount advised by Dr. King Chambers. I am quite sure that taking such diets as his makes women ill. These estimates differ so widely that one is surprised at the difference. Why, in the Royal Navy, where men are freely exposed to the oxidising influences of the ocean air, and are besides engaged during much of the time in doing laborious work, the full allowance of food is about three pounds daily, viz., one pound of fresh meat, half a pound of vegetables, one and a quarter pound of biscuits, two ounces of chocolate, a quarter of an ounce of tea, and one-eighth of a pint of spirit. It is said, though I do not know with how much truth, that the ordinary seaman does not use so much food as this, and that, when

left to follow his inclinations, he takes not three pounds of food but 30 ounces on the daily average, or about two pounds. I must say that to eat one and a quarter pound of sea-biscuit daily is a task that the ordinary man might well shrink from; and one shudders to think of the indigestion, constipation, acne, and rheumatism it must cause, when taken to such an extent, and in addition to other food.

Another point of great importance and, on the surface, of some considerable difficulty, becomes cleared up by this line of observation and reflection. The first effect of a fast is said to be to increase the number of the corpuscles of the blood. Now as the function of food is to make blood, and as the food therefore makes the corpuscles, it seems at first sight impossible to conceive how stopping or greatly diminishing food supply can increase the number of the blood corpuscles. It ought to diminish them, and in fact, in course of time, if the fast is persisted in, it does diminish them. But the explanation is really very simple. Most of us are over-fed, and the consequence of this is that the tissues are blocked or choked, because too much material finds its way into the blood. This directly prevents the blood-making processes from going on. The process is checked because the capillary vessels, the lymph spaces, the lymph ducts, and the muscular coverings, and the tissue coverings, and the connective tissues generally, are blocked. Consequently there is an accumulation of waste unused material in the body. This accumulation often lowers the

temperature and prevents and checks the accomplishment of all the processes of the body, and, among the rest, of the important process of the manufacture of the blood corpuscles. But when a fast is instituted, the body must live on something, and it calls up into the capillary and lymph circulation minute particles of tissue and of accumulations which had not yet become tissue, and oxidises these in order to maintain the bodily heat and energy. The consequence of this is, by gradually removing the block from the tissues, to allow the blood-making process (and other processes also, no doubt), to proceed for a time more healthily and more vigorously, until in course of time — it often takes a few weeks for this to happen — all the unused stuff has been made use of, after which the corpuscles begin to diminish. If the fast is pushed to actual death, of course all functions and all processes come to an end, and, long before the man dies, there is no blood formation going on in his body. But the explanation of the difficulty that fasting should even at first increase or seem to increase the number of the blood corpuscles, when prolonged fasting certainly diminishes them, becomes really quite easy. It depends on the economical tendency of nature to accumulate reserve stocks in the body, so that under any stress or strain she shall be able to go on for some time even without the ordinary and regular supplies of food. Incidentally we also see how over-feeding lowers the bodily temperature, and how fasting at first raises

it to normal, and may even by reaction raise it above normal, or may cause fever, after which, of course, it, like the blood-making processes, falls, and is checked by fasting. And we also see how it is important that some reserves should always be present in the body so that they can be drawn on in case of need. But the great test as to whether the reserves are in excess is the state of the temperature and of the other functions of the body, which. as we have seen, are reduced by too much. They are so reduced, at least, when the body is tolerant of the irritation of too much, although not infrequently we find the opposite condition set up by the same cause, viz., irritation with intolerance or reaction, when we get feverish temperature and almost all of the functions heightened or elevated for a time.

This state of body explains some otherwise inexplicable conditions. I have, for example, referred many times in the course of these observations to fasts which have lasted, not for hours or days, but for many weeks ; after which, in numerous cases, patients have recovered from severe and long-continued illnesses. With these facts before us, what are we to say of a statement that, for example, English soldiers actually died (it was freely said of starvation) after being exposed to the fatigue of military manœuvres for eight hours in the heat of an English summer at Aldershot, and after being deprived of food for eight hours? That they unfortunately died is not to be questioned; but they certainly did not and could not have died

of starvation. But if they had been over fed before, or if they had taken much alcohol, then the waste unassimilated stuff within them might easily go into excessive combustion or oxidisation; an acute and feverish poisonous condition might be set up, which might easily cause severe illness and even death. And the remedy, the true remedy, would be rather the inculcation of such food habits (and drink habits) as would render the body healthy enough and strong enough to bear easily such an amount of fatigue as they might expect to have to undergo at any time in actual war, than to condemn the War Office and the military authorities for their barbarity in not supplying the men with bovril, &c., while on the march. Nevertheless, the outcry in the public prints a couple of years ago took only the latter form, while not one word was said in condemnation of those previous habits, which, there could be no doubt, led to the catastrophe. And the chief trouble is that the sufferings of the soldiers, and the loss of valuable lives to the country, are certain to have been in vain, until we learn what the causes of both were, and until we take proper steps to obviate them. Because, after all, it is very doubtful whether the order in future to administer refreshment to soldiers on the march will have the effect expected. Besides all which, of what use to their country, it may be pertinently inquired, are soldiers who are so weak that they cannot march eight hours in warm but not tropical weather without halting to take refreshment? Would an

enemy be so considerate as to intermit their attack while our men were so employed?

It is, then, the man with reserves in him from over-feeding, whose corpuscles become more numerous from fasting (for a time), and it is only when the process of accumulation of reserves has been carried to excess, that the condition has become unhealthy. The conclusion of the whole matter is—what everyone knows and admits theoretically, but hardly anyone carries out into practice—that moderation is the only fixed and the only unfailing rule. *Ne quid nimis.* *Μηδὲν ἄγαν.* Nothing too much. There is nothing new in this. It has been all said before. And yet the translation of it is somewhat new. For what is moderation? That is the question. Well, moderation differs in different circumstances; but for the average man and woman living in a town, that is for the average man and woman in England to-day (and in most other countries, too), moderation is much nearer to the amount of food mentioned by Cornaro, the 12 ounces of mixed food as it comes to the table, with the 14 ounces of red wine, than it is to the scale, or rather the widely varying scales, of some of the modern physiologists. I am certain at least that this is so, if the aim of medicine is to be, as I think it ought to be, to increase, and lengthen, and make more vigorous for active work, and also, while it lasts, less susceptible to illness, the life of man on this planet. Only he knows or can appreciate the discouragement caused by looking on and seeing active and useful human

organisms rendered, sometimes quickly and sometimes slowly, inactive and useless by being silted up and choked with too much food—only he can adequately appreciate the discouragement and sorrow caused by the loss, one after the other, of friends or acquaintances, who has been forced to reflect that such loss might easily in most cases have been postponed by from five to twenty years, if more rational ideas had prevailed on this simple subject, and if, prevailing, they had been translated into the conduct and habits of everyday life. The causes of death seem to be attributed to every reason but the right one. It is bronchitis, it is pneumonia, it is Bright's disease, it is cancer, it is tuberculosis, it is apoplexy; but the causes of these causes—who says anything about them? About the drinking (yes, we are alive to that), but particularly about improper feeding as a cause of the causes of too early death—how is it that we are all so blind to this? How many young lives are sacrificed on the altar of over-feeding? Well, I am glad to see that our medical officers of health are beginning to warn us of this. One is apt to be accused of extravagant statements if one attempts to answer this question. In time, however, the labours of our medical officers of health will tell in this direction. But how many wives have been made widows; how many husbands have been left wifeless; how many both wives and husbands have gone to asylums, and how many children have been left orphans from this cause? It were

much to be wished that we could induce our people to ask and answer these questions. If only a small part of the ability so abundant in life were directed to the consideration of the simple questions, how much food is necessary and desirable for the adequate but not excessive nutrition of the body of the average man, and woman, and child, and how often should that food be taken ; what a harvest of information and what a harvest of happiness would be reaped. If our Medical Council, instead of crowding the medical curriculum with more or less useless knowledge (all knowledge is useful, no doubt, and I do not despise any of it, but some forms of knowledge are much more useful than others) about bacteria, and germs, and micro-organisms, would insist on instituting a chair of dietetics in every medical school; or, perhaps, better still, if our Municipal and County Councils would found lectureships on this simple subject, so germane to practical life ; and if the occupants of such chairs and if such lecturers would tell the people what, and how, and how often they should eat and drink, and what are the effects of improper eating and drinking, the effects would, I am sure, be such as to astonish us all by their magnitude, and importance, and far-reachingness.

There are still one or two questions remaining for consideration before we can complete the discussion of the question how often ought we, *e.g.*, to eat in order to be well, and fit for work. There have been very different practices followed in this

respect by different nations; but a considerable amount of additional knowledge has been gained regarding it of late years. As to the time required for the digestion of food, and as a consequence of this, the number of meals which it is proper to take in a day, much difference of opinion and of practice prevails. It seems to me unfortunate that many of the data as to the time that food remains in the stomach before it is passed in a partially digested form through the pylorus into the small intestine, have been obtained from the experiments of Dr. Beaumont on the stomach of St. Martin. St. Martin was a young Canadian who, about 1830, received a gun-shot wound in the stomach. This healed, leaving, however, a valvular opening, by means of which it was possible for Dr. Beaumont to examine at various times, after food had been taken, what amount of digestion had been performed; and careful observations were made by Dr. Beaumont on this point. Thus, to take an instance: "At three o'clock on a certain day, St. Martin dined on boiled dried codfish, potatoes, parsnips, bread, and drawn butter. At half-past three o'clock Dr. Beaumont examined and took out a portion about half-digested; the potatoes the least so. The fish was broken down into small filaments; the bread and parsnips were not to be distinguished. At four o'clock, examined another portion. Very few particles remained entire. Some of the few potatoes were distinctly to be seen. At half-past four o'clock he took out and examined another

portion; all completely chymified. At five o'clock, stomach empty." "Dr. Beaumont constructed a table shewing the times required for the digestion of all usual articles of food in St. Martin's stomach, and in his gastric fluid taken from the stomach. Among the substances most quickly digested were rice and tripe, both of which were chymified in an hour; eggs, salmon, trout, apples, and venison, were digested in an hour and a half; tapioca, barley, milk, liver, fish, in two hours; turkey, lamb, potatoes, pig, in two hours and a half; beef and mutton required from three hours to three and a half, and both were more digestible than veal; fowls were like mutton in their degree of digestibility. Animal substances were, in general, converted into chyme more rapidly than vegetables." I do not deny or question the statement that these facts were true for St. Martin's stomach; but I am very sure that whoever should infer that because they were true in that particular case, they are true in general in the case of the average man living in a town and doing a townsman's work, would make a very great mistake. The cases are not comparable. We ought not to assume that the activity of digestion in a young Canadian backwoodsman, living an active and even laborious life in the country, will be parallelled by the processes occurring in the digestive apparatus of a man doing office work or the work of a business man in a town. Whoever should imagine that, because the backwoodsman's stomach was completely empty

in two hours, of a meal of codfish, potatoes, parsnips and bread, the same rapidity might be expected in a townsman, would make a disastrous mistake. According to that, it would be quite justifiable to eat in four hours again, that is, at seven o'clock; and this would allow two hours for the chyme to become chyle, and to get into the blood and be used there. Perhaps, even in this case, five hours would be a better interval between meals than four, since we do not know quite how long a time the latter processes require for accomplishment, and it is well not to overload the digestion or to clog the lymph-separating processes. But there is the most unmistakeable evidence that such a meal as St. Martin took is, in many cases, not out of the stomach of a townsman for five or six hours after it is taken; and I have known it longer. For instance, I have seen, and that not once but many times, food eructated from the stomach, and in such a condition as that some of the different materials of the meal could be distinguished from one another, quite six hours after it had been taken. Food taken at two o'clock, for instance, has been seen by me even as late as eight o'clock, and parsnips in particular have been observed. There is a minority of persons who eructate their food for hours after they eat it, performing a process not very unlike the chewing of the cud in herbivorous animals; and I have had more than one such person under observation for long periods of time, so that I know that in such persons it is the rule that stomach digestion

requires, not two hours, as in St. Martin's case, nor even three or four hours, but five or six hours, or even longer in some cases. Of course, I do not mean that no food leaves the stomach for the intestines under this length of time. The fatigue, sudden sometimes, which is so noticeable three or four hours after a full meal, whose explanation is given later, shews that some of the food must do so, but in such persons, not infrequently the whole of the food taken at a meal such as dinner, has not left the stomach for quite five or six hours, or even longer. In pathological conditions the time is often much longer than this. I have known food to be in the stomach for as long as 48 hours; in fact, some sorts of foods, as, for instance, the cocoanut, which is not only indigestible, but irritant to the stomach, not infrequently require this length of time for removal. The mushroom, again, which, though indigestible is not irritant, but bland, not infrequently lies in the stomach for 24 hours even in normal circumstances; and I have known a cup of cocoa lie for 36 hours in atony of the stomach, a condition of disease, of course; but the facts as to the cocoanut and the mushroom are natural and physiological and normal in many instances. I saw on one occasion in a patient with slow digestion (it is true the salad had rather too much vinegar in it), a salad which had been eaten at 7-30 p.m., vomited in exactly the state in which it had been eaten, so far as appearances went, at five o'clock next morning, or say nine and a half hours after it had been

taken. The curious thing was, and I believe that this is not at all an uncommon occurrence, that all the other articles of food taken at 7-30 appeared to have left the stomach, since none of them were recognised. It appears to me from this and other like experiences, that the stomach has the power of selecting what it can digest, of digesting it, and of passing it through the pylorus into the small intestine, while it retains within its walls other and undigested, and perhaps we may say, also, indigestible, foods. But if, as in the previous case, we add three hours as the time required for food which has left the stomach to get into the blood, so as to be used there for purposes of nutrition, and if we take even the shorter period of five hours as that required for complete chymification by the stomach, persons whose digestion is so slow as this ought not to eat again for at least eight hours after their previous meal was taken ; and I believe that even a longer interval is often demanded if health is to be maintained. Such persons might eat, *e.g.*, at eight a.m. and four or five p.m., and would generally be well nourished on two meals a day. In fact, I have often recommended men, whose digestion is so slow as this (and it is a much commoner case than is usually supposed), to eat not even twice a day, but once only, so as to let the digestion of one meal be completely effected, both its primary digestion in the stomach and intestines, and the secondary digestion or lymph assimilation in the tissues, before the next one is taken. I have also to

add that in the cases in which this advice has been accepted and acted upon, much benefit has accrued to health and vigour in almost every instance. I have recommended this course at all adult ages, up to 86 even, and with great benefit. I have not prohibited a hot drink of tea, coffee or cocoa twice a day in addition, but have preferred that it should be taken without any bread, or perhaps with half an ounce only. An interesting and somewhat curious thing in reference to tea and coffee in this connection has been the fact that while persons living on the ordinary three or four daily meals have alleged that they could not take tea or coffee after five p.m. without lying awake till three or four in the morning; after they have gone on to one daily meal the same persons can sleep soundly on tea or coffee taken as late as eight or nine or even ten p.m. Similar interesting and curious facts have appeared in regard to other articles of food. Thus many persons think they cannot take cheese because of its disagreeing with them, or because it constipates them when they live on three or four daily meals. But the same persons, on becoming monositeous, or taking only one daily meal, have found that they could quite well take an ounce or even two ounces of cheese, without discomfort, and, more remarkable still, have thought that, in place of causing constipation, it helped the action of the bowels. This kind of experience it is which has gone towards forming in my mind the conclusion that it is not this, that, or the other

article of diet which makes us well or ill, but that health or discomfort depends rather on how much we take, and how often we take our food.

While I am on this subject, I perhaps ought to mention that although the recommendation to eat once a day only has for the most part in my practice been offered to persons after middle life, as being most suitable to the condition of those whose bodily frames have been fully grown, and often, indeed, over-grown, it has not been altogether confined to them. I have often recommended children to eat twice a day, and always with advantage; and in one case I recommended the mother of a child of four years, who was suffering from a tuberculous disease of the knee joint, with suppuration in the joint and necrosis (or limited death) of the thigh bone, to let her have only one meal a day; and that with the best results. I ought to say that the child had also a glass of milk each morning and each evening, but no solid food at either of these times. Milk is, no doubt, food, but the number of calories represented by a glass of it is only about 140, so that the two glasses represented about 280 calories. According to the latest authorities, a child of four years of age ought to have such a quantity of food as will produce 1200 to 1400 calories daily, so that 280 calories represent only a very small proportion of this. The child to whom I refer had only as much food as would produce at most 850 calories when converted into energy in the body, and probably

800, or even less, more nearly represented the amount which she did have. Nevertheless, on this diet, which, according to the authorities, was a restricted diet, or even a starvation diet, but which proved to be nothing of the kind, the child grew nearly ten pounds avoirdupois in weight in fifteen months ; her bowels, which had been constipated, became perfectly regular ; and the suppuration in her knee joint was entirely healed. Could anything shew more strikingly than this case that the quantity of food recommended by the physiologists working in laboratories is much greater than is required by a child in actual or real life ? Among a large number of criticisms which the publication of this case called forth, most of them, strange to say, laudatory (but general opinion is, I think, fast altering on this subject, and people are at least not so intolerant and contemptuous as they used to be, and that not long ago, of views such as I am here advocating), I received one from a medical friend of over 30 years' standing in the profession, and he suggested that I had given the child not too little but too much ! ! I had myself hinted in my pamphlet on the subject, that, much as I had restricted the child's diet, who had had little more than half of what the latest authority said she ought to have had (and this treatment had been continued for fifteen months), I doubted if I had restricted it sufficiently. Dr. Haddon wrote to say that in his opinion I had given her more than she required. I must say that I agree with him, but if this is a correct view, how

flagrantly out must be the physiological authorities who recommend so very much more. The difference between us is not as to a little less or a little more, but it is a difference of two to one, or even perhaps three to one. Well might Hippocrates say that the issue could not be determined to a nicety. Apparently it cannot be determined at all—but this is absurd. One of the estimates must be so far away from nature as to be wrong. Which is it? Day and night no doubt shade off into one another by insensible gradations, so that at a given point of time it is difficult to know whether it is day or night. But for all that, the ordinary man and the ordinary child has no practical difficulty in distinguishing between the light of day and the darkness of night, and doctors and physiologists (for the practice of medicine is or ought to be—I repeat it — only applied physiology, and pathology is only disordered physiology, or at least is mainly so) ought to be able to come nearer to a practical conclusion as to whether a child ought to have for health as much food as is represented by 1400 grand calories, or only by as much as 800 or 700 or 600 or 500 a day. It is said of the great Hippocrates himself that he entirely failed to please all parties. No sensible or self-respecting man would try. On one occasion, being accused, however, by one party, of feeding patients to death, and, by another, of starving them to death, he made no reply, but allowed the one criticism to answer the other. The medical man

who to-day attempts to hit the golden mean of moderation between too much and too little, will probably fail to please either party, one of whom will be apt to say one thing of him, and the other the opposite ; but he may take comfort in the reflection that he finds himself in the good company of those who in the past have attempted the same task ; and he may also anticipate that, in the future, the history of his efforts, and perhaps his failures, will comfort the heart and cheer the courage of successors who will find themselves attempting the same arduous enterprise. I think in this matter that Dr. Haddon is much more nearly correct than some of the physiological authorities, and the great majority of the medical profession, who, afraid seemingly to verify things for themselves, so slavishly follow their lead. The happy issue of this case under the treatment adopted, a treatment which I have used not only in juvenile struma, but also, and that much more than once, in senile struma also, has confirmed me in the view I have often expressed, that the most successful treatment of tubercular disease is that founded on the view that its management is analogous to the drainage of marshy land in agriculture. More water is getting into the ground than is flowing out, and the problem is, for a time, and until proper drainage has been effected, to allow more to get out than flows in, after which time proper drainage must be resorted to. In white swelling of the knee or other joints, a precisely analogous

problem presents itself for solution, for much more lymph is finding its way into the joint structures than is flowing out, and the problem therefore is, and must be for a time, and until a proper balance has been effected, to allow more lymph to get out than flows in, after which time proper feeding must be resorted to. If this view is in collision with the present orthodox opinion, fostered and supported by that love of indulgence, and of luxury, and of short-lived pleasure, which is so characteristic of our time, the orthodox opinion that cases of tuberculosis ought to be stuffed or over-fed, I can only regret that such evidence as I have has forced me to a different conclusion. The serious question, fraught with the issues of life and death, and of maiming or soundness to our patients, is, which view is nearer to the truth. I may say that the mother of the child in question, finding the results so satisfactory in one child, and finding that an elder sister, though three years older, weighed only two pounds more than the younger one, and finding that, although not ill, she looked pale and anæmic (triphthæmic or catatribæmic, *τρίβειν* = to rub or waste) of her own accord has put the elder child on to the same lines of treatment as the younger one. I cannot doubt what the result will be. The elder, also, will get a better colour and gain weight. The law having been pointed out and declared by the medical interpreter of nature, all that the patient has to do is to attempt to obey

it, and the more nearly he can do so, the better health and vigour will he enjoy. If any hitch arises or if any difficulty occurs, the medical expert may again be consulted; but his proper function must in that case again be to clearly point out the scope of the law and clear up any misconceptions in its interpretation. All that the doctor can do is to declare the law; all that the patient can do is to obey it; and the more nearly the patient succeeds in doing this, the less and less necessary does the medical expert become. If we know the formula by which the law of gravitation acts on a falling body, we do not need a mathamatician or a physical expert to make its application in any given case. With some knowledge of arithmetic we can do that for ourselves.

The complaint which patients not infrequently make to their doctor, of feeling tired several hours after taking a meal, may now be shortly considered. If, for instance, they dine at half past one or two o'clock, they feel very tired, and sometimes rather suddenly so, especially if the meal has been a somewhat full and large one, about five or six o'clock. How is this they ask? Well, the explanation is very simple. In three or four hours after eating, digestion has made some progress towards effectiveness. Chyle from the food has begun to be poured into the blood. If the chyle so poured into the blood bears too large a proportion to the total volume of the blood, the in-rush is too great, and when the blood, laden in this way with too much

chyle from the thoracic duct, finds its way to the muscles, which, as we saw before, may contain some 30 per cent. of the blood in them, they become rather suddenly loaded with too much material, and being oppressed with this, they cry out as if fatigued. Especially will this be so, if, the meal having been a large one, it has not been very perfectly assimilated. The chyle in this case will have been imperfectly elaborated, and the blood therefore imperfectly made. The blood, finding itself loaded with imperfectly assimilated stuff from the food, will begin to get rid of it into the muscle sheaths; and the lymph spaces, being somewhat choked with excess of lymph, and particularly with excess of lymph from imperfectly assimilated food stuff, the lymph vessels will not carry away the lymph plasma sufficiently quickly, and, as a certain amount of swelling will therefore occur, the person will feel tired. Neither the muscles nor their sheaths will work smoothly or easily, and the sensation of fatigue is really caused mainly because the muscles are loaded with bad blood containing ptomaines and other unassimilated materials. The blood is said to contain uric acid. No doubt it often does, but it is very unlikely that uric acid is the only poison carried by the blood to the tissues in such circumstances. No doubt other materials besides uric acid are present, some of them known to chemists as, *e.g.*, lactic acid, and very probably others as yet unknown. In fact, uric acid stands mainly as a symbol or sign of imperfectly oxidised

and imperfectly assimilated products of digestion, and it is these in their totality or aggregate which poison and fatigue the muscles and the whole locomotor mechanism. But the blood received its excess of material, too much in volume in proportion to its total capacity, and ill-assimilated besides, from the thoracic duct. The thoracic duct in turn received its contents from the chyle of the small intestine. (The contents of the thoracic duct have sometimes been described as the lymph from the villi of the small intestine. This terminology is illuminating, and instructive of it makes us think of the meaning of the purposes which lymph performs in the body). The small intestine in its turn received its contents from the stomach through the pylorus; and the stomach in its turn received its contents by the oesophagus or gullet, which conducted the food often far too quickly, and far too imperfectly masticated, from the mouth. The whole processes of digestion thus hanging together, and being dependent, step by step, and each step on the step immediately preceding it, too much fatigue after eating, whether an hour after, or three or four hours after, which is a far commoner experience, calls out to us rather (if our ears are attuned to listen) to masticate properly and to eat less, than to lie down for an hour after eating, or to lie down for two hours, or say three or four hours after food. And when at 5 or 6 o'clock a cup of afternoon tea is taken, the stimulus it gives is so marked as to be an almost universal experience, whose good

effects, if people would but make the experiment, would be as beneficially experienced, and much more safely, without any solid food being added with it.

I fear I shall get on to very controversial ground in what I am going to say next. I do not recommend the taking of alcoholic stimulants at all; but *if they are taken*, I recommend the average man to take only as much alcohol as would be represented by one small glass of whiskey twice a day. More than this transcends the bounds of moderation. But now if he is going to take it, say at five in the afternoon, or at eleven in the forenoon (not so suitable a time, I think), *if* he is going to take it, ought he to eat with it? Well, I think not. Why should he eat at either of those times? He does not require food at 5 p.m. if he has had lunch at from 1 to 2 o'clock; and if he does not take his first meal till 12 or 1 o'clock, he still ought not to eat at 11, because whatever he eats then will certainly not be digested when he has his meal at 12, or 1, or 1-30. The same reasons which justify the advice not to eat with afternoon tea compel one, therefore, to offer the advice that if a person is going to take alcohol either at 11 a.m., or 5 or 6 p.m., he should refrain from eating at those times. It seems to be thought that if a man eats at the same time that he takes alcohol, the food will prevent the alcohol from doing his stomach so much harm as if he takes it alone. I think, however, that this reasoning is fallacious, and that the habit

of too frequent eating, which such practices confirm, does an immense amount of damage by keeping up continual demands for work on the part of the digestive organs. If, therefore, a man does take a little alcohol at either of those times (which I do not recommend him to do) I think he ought not on any account to eat at those times. And if his second ounce of whiskey is taken at bed-time, as it so often is, I for the same reason advise him not to eat biscuits or any other food at that time. Frequent eating and frequent alcoholic drinking are two very bad things, but it is certainly more dangerous for a man or woman to do both together than it is to do one alone.

Another question which patients often put to their doctors is this : Why do we, they will say, wake tired in the morning after six, seven, or eight hours' sleep? The answer to this question need not detain us long. We wake tired in the morning because too much material has been finding its way into the blood from the digestion, and because during the quiet of sleep the blood has taken the opportunity to drop in the connective tissue that excess of material which was oppressing it. As the connective tissue forms the coverings of the muscles, bones, joints and nerves, the consequence is that the whole locomotor system is in such circumstances over-loaded, so that whenever we begin to move we are tired. During sleep I believe the veins are dilated, and the arteries contracted. The longitudinal elements of the former vessels are stimulated more than their

transverse elements, while the transverse elements of the arteries are contracted rather more than the longitudinal. (When the reverse of this occurs, *i.e.*, when the arteries are dilated, the person is sleepless). The veins being thus in sleep dilated and full of blood, which is, besides, moving rather sluggishly, the blood has every facility of getting rid of any material it may contain in excess. Another form of the question often is: Why do we sleep for say two hours, then wake, and find ourselves unable to sleep again for say two or three hours, or perhaps not at all; or perhaps we fall asleep just when we ought to be getting up? And yet another form of the question may be: How is it that we cannot sleep till three, four, or five in the morning? And still another form of the question may be: Why do we sleep too heavily? After the discussions in Chapters VI. and VII. we can readily understand that defect of the function of sleep, irregularity of that function and excess of it, are all due to the same cause, viz., that we are eating too much or too often, or that we are eating too much *and* too often. I do not know if my explanation of the respective state of the veins and arteries in sleep is correct or not, although I think it is. I consulted an eminent physiologist on the point, and he said it was a very difficult question to answer. But it does not really matter whether the precise explanation offered is the correct one. What does matter is that during sleep the blood does part to the connective tissue with materials

which it contains beyond its needs, and that this physiological fact accounts for the feeling of fatigue which many persons experience on waking in the morning. The connective tissue is what we may call the dumping ground of the blood ; it is the safest or the least hurtful place in which waste products can be deposited, for if they were not deposited in the ilemmatous parts of organs, they might be dropped into the parenchymatous portions, in which case, instead of being tired simply, we might get an attack of pneumonia. As the muscle sheaths, the bone coverings, or periosteum, the joint ligaments, and the nerve sheaths, as also the brain-and cord-membranes are all made of this connective tissue, all the locomotor system is oppressed and weighted by being thus made the dumping ground of the blood, and therefore whenever one moves after waking in the morning, one feels and must feel tired. There is really inflammation or at least congestion all over the body, for the connective-tissue is found practically everywhere in the body. No wonder, therefore, that the body is tired. Every movement is clogged, and the patient feels as if she could not drag one leg behind the other. I have suggested that this condition should be called *Initis*, from the Greek *ἶσ*, Latin *vis* = strength. Homer used the word *ἰνίον*, as did also Hippocrates, of the strong, firm elastic tissue at the nape of the neck, the same which, immensely strengthened in the horse, for example, enables it to carry its heavy load almost at right angles to its

body (or take the elephant as even a more striking example), seemingly without getting tired or knowing even that it has such a heavy weight to carry. Even in man, however, the tissue is strong; and a marked sign of advancing age and of danger to life is noticed when this strong tissue begins to get weak, and the head begins to shake or tremble on the neck. Old persons seldom last more than two years, I have observed, after beginning to manifest this sign. Sometimes the affection of the connective tissue in young people is called *hysteria,* sometimes *neurasthenia,* sometimes *neuralgia,* and sometimes women (for it is oftenest observed in them) are reproached with being able to avoid complaining if they would; and sometimes, I regret to say, are recommended to undergo many and varied mutilating and other operations on account of it, which operations do not and cannot cure them, A further stage of *initis*, or inflammation of the connective tissue, as it is the further stage of all other inflammations, is effusion of fluid, sometimes into joints, when it is called *synovitis,* or if many joints are affected, *rheumatic fever;* or if the effusion takes place into the pleura, it is called *pleurisy;* or if into the peritoneum, it is called *peritonitis;* or if into the brain membranes it causes *coma,* or unconsciousness, with dilated pupils, and perhaps death. It is quite true that these very serious developments occur only rarely, but they do occur often enough to make practitioners wary, and the older a doctor gets the less lightly does he

which it contains beyond its needs, and that this physiological fact accounts for the feeling of fatigue which many persons experience on waking in the morning. The connective tissue is what we may call the dumping ground of the blood; it is the safest or the least hurtful place in which waste products can be deposited, for if they were not deposited in the ilemmatous parts of organs, they might be dropped into the parenchymatous portions, in which case, instead of being tired simply, we might get an attack of pneumonia. As the muscle sheaths, the bone coverings, or periosteum, the joint ligaments, and the nerve sheaths, as also the brain-and cord-membranes are all made of this connective tissue, all the locomotor system is oppressed and weighted by being thus made the dumping ground of the blood, and therefore whenever one moves after waking in the morning, one feels and must feel tired. There is really inflammation or at least congestion all over the body, for the connective-tissue is found practically everywhere in the body. No wonder, therefore, that the body is tired. Every movement is clogged, and the patient feels as if she could not drag one leg behind the other. I have suggested that this condition should be called *Initis*, from the Greek *ἶσ*, Latin *vis* = strength. Homer used the word *ἰνίον*, as did also Hippocrates, of the strong, firm elastic tissue at the nape of the neck, the same which, immensely strengthened in the horse, for example, enables it to carry its heavy load almost at right angles to its

body (or take the elephant as even a more striking example), seemingly without getting tired or knowing even that it has such a heavy weight to carry. Even in man, however, the tissue is strong; and a marked sign of advancing age and of danger to life is noticed when this strong tissue begins to get weak, and the head begins to shake or tremble on the neck. Old persons seldom last more than two years, I have observed, after beginning to manifest this sign. Sometimes the affection of the connective tissue in young people is called *hysteria,* sometimes *neurasthenia,* sometimes *neuralgia,* and sometimes women (for it is oftenest observed in them) are reproached with being able to avoid complaining if they would; and sometimes, I regret to say, are recommended to undergo many and varied mutilating and other operations on account of it, which operations do not and cannot cure them, A further stage of *initis*, or inflammation of the connective tissue, as it is the further stage of all other inflammations, is effusion of fluid, sometimes into joints, when it is called *synovitis,* or if many joints are affected, *rheumatic fever;* or if the effusion takes place into the pleura, it is called *pleurisy;* or if into the peritoneum, it is called *peritonitis;* or if into the brain membranes it causes *coma,* or unconsciousness, with dilated pupils, and perhaps death. It is quite true that these very serious developments occur only rarely, but they do occur often enough to make practitioners wary, and the older a doctor gets the less lightly does he

tend to treat those so-called hysterical affections, even although very few seem to have made out the pathology of them ; for every experienced doctor must once or oftener have been compelled to see a calamitous issue to cases of this kind. And if he has spoken slightingly of the case, and has suggested, for instance, that the young woman should have water poured from a jug on her upper lip between her nose and mouth, so as that by preventing her from breathing properly she may be roused out of her "hysterical" state, and if she should unhappily become comatose and die, his credit is severely diminished and justly impaired, and the value of his advice to the friends not to make too much of the ailment, and "not to be too sympathetic," is severely called in question. For the curious thing about these cases is their variability. Sometimes the women who suffer in this way are now quite well, and then again, and that in a short time, very ill. Alterations in the circulation of the blood, and particularly, I think, of the lymph, alterations which may and do occur suddenly and extensively and unexpectedly, make the women now very ill when connective-tissue-lymph-congestion occurs, and in a short time they seem to be much better again, when connective-tissue-lymph-relief occurs ; and the alternation of these processes taking place at short intervals of time, gives to the disease that erratic character which has led to so many mistakes being made about it. It seems to me to be a very simple thing

to understand it, and, understanding it, to treat and cure. Unhappily, the advice which is good for it goes against popular, and, worse still, against professional opinion, so that on all sides, from sympathetic and well-meaning but injudicious friends on the one hand, and because the medical profession do not always seem to have clear ideas regarding either the nature of the affection or its cure on the other, the patient is dissuaded from taking the advice to restrict her diet, which, nevertheless, is the only advice (that and gentle and cautiously increased exercises) that can do her any good. Pain between the shoulders (the muscles of the back being sore and tender) and in other parts of the body, and what is very badly called anæmia or chlorosis, are frequently concomitant conditions. Also, as we saw before, we find constipation present, which is too frequently treated as if it were the substantive affection, whereas, in fact, it is a concomitant condition, due, as we then saw, to the same plugging or blocking of the lymph circulation in the intestine, which, when it exists in the brain-connective-tissue, causes all the flightiness and disorder of mind and body which characterise this very grave and by no means imaginary complaint. Recurring headaches, megrim, neuralgias, &c., &c., also often characterise it, very difficult to understand when improperly viewed, and yet so simple and so easy to treat and to cure when one can manage to persuade patients, and particularly to persuade their friends, to allow to be used the only means that are

suitable for them. In some cases indigestion is the most prominent sign, and it is happy when this is so, for then there is some chance that the patient and her friends will submit to treatment; and also the medical adviser has his attention perforce directed to the real cause of the ailment, which manifests itself in so many and varied and protean forms and symptoms. Cases of this kind are the despair at once of the public and of the medical profession, and yet they are easily treated when properly handled. Such a case I saw recently in a young woman (like all my colleagues I have seen scores, perhaps hundreds, of them — they are very common indeed), and handled with perfect success, by persuading her relatives to allow her to be submitted to a much restricted diet for a fortnight, that is to say, the whey from one pint of milk daily, after which, for another fortnight, she was put on one meal a day, and recommended to have exercises first once and then twice a day; and for the third fortnight she was allowed two meals a day, the first at 12 noon, and the second at 7 p.m. I also ordered a large linseed meal poultice to be put over the abdomen for an hour morning and evening at first, and afterwards in the evening only. The girl, who had been ill for years, was entirely reasonable herself, and having suffered so much and so long, was only too glad to submit to any treatment likely to benefit her. It was the dyspeptic form of hysteria (or, as I prefer to call it, *initis*), from which she suffered. She vomited

everything she took, or almost everything, and was thin, pale, emaciated, and anæmic (triphthæmic); she was suffering, in fact, from indirect starvation, the proper treatment of which is, at first, restriction of the diet, as we have seen, and then cautiously increased feeding as the patient can bear it. It is really one of the simplest things to understand and to treat if only it is properly viewed, but, when improperly viewed, one of the most difficult and the most intractable. But, in one word, the common error concerning it is the mistaking of indirect for direct starvation, and the treating, therefore, by increase of food what ought to be treated by restriction. It is a very serious error, which even experts sometimes fall into. When the head is very much affected in some of these cases, the connective tissue about the brain being much irritated, patients not infrequently lose the balance of their minds and reason, and even have to be put under care. Unreasonableness, flightiness, uncertainty of disposition, a mixture of amiability and perversity, and a curiously subtle power to excuse and defend eccentricity, and to make the worse appear the better reason, characterise the mental condition of persons suffering in this way; added to which is often found a morbid desire to nurse and even create grievances, some of the women seeming never to be happy unless they have a grievance. Sometimes love is said to make them go wrong, sometimes religion. The fact is, they are so disturbed that anything would or

might have upset the balance. Love or religion may be the *occasion*, no doubt, but the condition of the nutrition is the main *cause*. The connective tissue inside the head has become congested from improper nutrition, and has irritated the brain cells, preventing their healthy action. I mention this here particularly because so much good can often be done in such cases by restriction of the diet. I have already mentioned the case of a lady who recovered rapidly when she braced herself up to follow the advice given her to stop the inordinate quantities of sweets which she had been in the habit of taking. And I have also referred at some length to Dr. Dewey's case, in which a lady fasted for 45 days, so restoring herself to perfect sanity. These cases emphasize the question asked by Dr. Dewey, whether we might not anticipate better results than we now obtain if we treated insanity more frequently by restriction of the diet. Certainly no patients could be so thoroughly controlled as patients in asylums, and it would always be easy to interfere if there seemed to be any danger to life from direct starvation. Sometimes, no doubt, the connective tissue in other parts of the body, as, for instance, the pelvis, is inflamed also in these initic affections. It is natural that it should be. The same blood goes everywhere in the body, and, being badly made and loaded with unused stuff, may deposit this stuff, or some of it, in the connective tissue, wherever connective tissue exists, and therefore in the pelvis, causing various adhesions

and inflammations of the organs in this region of the body, or even sometimes going on to suppuration. But even such adhesions do not justify many of the surgical operations which are performed for their relief, and certainly do not justify the mutilating operations on women which are so common nowadays. The medical management referred to will nearly always in young women succeed in removing these adhesions, displacements, &c., if patients will be patient and intelligent as to their causation. And if operation is necessary, it is very seldom indeed that removal of organs is called for, especially as such removal too often fails to cure the complaints from which patients suffer. Evidently, unless something is said as to causation, the same processes must go on in the body, and must result in disease somewhere else, even if the particularly distressed part has been removed.

One other condition found in many of these initic cases I must not omit to mention, viz., the tendency often shewn to bruise easily. On the slightest occasion, as, for example, if they happen to knock against a piece of furniture, or if they sustain some very slight injury, and sometimes even when no history of injury can be made out, women will shew large black or blue bruises on various parts of their bodies. Sometimes, if a friend lays a hand upon them, takes hold of an arm, for example, the finger marks may appear next day. This condition was well known to the ancient

writers on medicine, and the Greeks called it *peliosis* or *pelidnosis* (πελιὸς = *black and blue, livid*), but I am not aware what explanation they gave of it, or if they gave any. It is evidently due to excessive weakness or to a very low degree of resistance in the connective tissue of the body, the coats of the finest blood-vessels being so weak as to become lacerated on sustaining the slightest injury or tension. Rupturing, they cause extravasation of a minute amount of blood into the cullular tissue of the body ; and it is the changes undergone by this blood in the course of its re-absorption which cause the discoloration and lividity referred to. But what is the cause of the excessive weakness of the connective tissue which makes it give way so readily ? Well, the cause is the one we have so often seen in action, mal-nutrition ; which, in nearly all cases, means over-nutrition of the connective tissue, and congestion of the blood and lymph circulation by the ingestion of (generally) small meals, too frequently repeated. This condition often coincides with the somewhat flighty and irregular mental condition to which I have already referred; and, indeed, women so improperly nourished not infrequently convert injuries sustained in the trivial ways described into proofs of cruelty, and shew them as evidence in accusations of ill-treatment. To advise persons suffering in this way to eat, not little and often, but little and seldom, if they want to be well in the comparatively inactive life which many of them lead, sounds, I have often been made

to feel, like adding insult to injury, but is, nevertheless, I regret to say, the only advice which can do them any good. The exercises which one might otherwise be disposed to recommend, they not infrequently cannot bear; in fact, such movements might only increase the evil, and the only thing left is that they should be advised to restrict their diet.

The conclusions, then, which seem reasonable from the evidence and considerations advanced in this chapter regarding the quantities and qualities of food suitable for the average man and woman, especially those dwelling in towns, are the following. The average townsman requires a quantity of from three-quarters of a pound to a pound and a half daily of mixed food as it comes to the table. The average townswoman requires a little less. About one ounce of food a day for every ten pounds of body weight, or, perhaps, in the case of growing children, every eight pounds of body weight, seems to be enough and not too much. According to this scale, a man weighing 140 pounds would require about 14 ounces of mixed food daily, and a man of 12 stones rather more than a pound avoirdupois. As to quality we require a mixed diet of proteids, fats, carbo-hydrates, and mineral matters; but whether we should get our proteids from animals or vegetables may reasonably be left to each person's judgment and (perhaps) conscience. The moral arguments against the slaughter of somewhat noble animals for food have always seemed to me stronger than the scientific ones. The man who

allows himself to think about the question usually inclines to take less and less meat. At one time, thinking that an excess of starch in the diet was the chief cause of illness, I inclined to too much diminution of the starch, and was therefore driven to the use of meat instead. For persons suffering from the effects of over ingestion of starch and sugar, the Salisbury diet of meat and hot water is no doubt often very useful; but its chief use, after all, I believe, is that it restricts the diet. To take four ounces of beef three times a day is a great reduction of food from taking two pounds or three pounds avoirdupois of mixed diet, the taking of which makes so many of us ill. But when we see that although it happens to have been, as an *accident*, starch and sugar which has made us ill, the real and *essential cause* has been too much food taken too often, we also see that it was an accident only, and that the reduction of our mixed diet to four or five ounces taken three times a day, with or without the washing out by hot water, would equally have enabled us to recover. It is easy, in these as in other matters, to mistake the accident for the property, to mistake too much starch and sugar for too much food, and, therefore, to recommend as a remedy the cessation of the former instead of a wise and cautious restriction of the latter. And, indeed, in the conflict of opinion and practice, there is some excuse for going wrong, or for failing to see all the bearings of the problem at first. A man who should take, for instance, three

pounds of beef daily, as some persons do, would probably soon find that he was taking too much food. As to calorie value, however, even three pounds of lean beef daily is only equal to about 2000 grand calories, or perhaps somewhat less. According to Mrs. Richards, a mere subsistence ration amounts to 2000 calories. On the other hand, Forster found that a lawyer at Munich consumed as much food as would supply 2400 calories, and a physician at the same place 2830 calories; while Ranke found that a university professor at the same place, with very little exercise, consumed as much food as would produce 2325 calories. These figures appear to me to go in the direction of corroborating what I said before as to the relation between plain living and high thinking, as also the physiological facts regarding the circulation in the brain and muscles respectively.

Then, lastly, as to times of eating, we conclude that no one ought to eat oftener than three times daily, say at 8-0 a.m., and 1-0 and 7-0 p.m.; that in many, if not in most cases, it is better to eat twice than even three times, say at 12-0 noon and 7-0 p.m., or at 9-0 a.m. and 6-0 p.m., if more convenient (in the latter case, a cup of hot cocoa or coffee about 1-0, with no solid food, would provide a good physiological stimulus); that to eat twice a day is suitable even for growing children, of whom, however, more will be said in a subsequent chapter; and, lastly, that in many cases it is better for the townsman to eat once a day only, rather

than thrice, or even than twice. In the last case, the best time for eating is about mid-day, or say from 12-0 noon to 1-30 or 2-0 p.m.; and a cup of tea or coffee may fitly be taken morning and evening besides, without any accompaniments; or, instead of the evening tea, a glass of milk with or without the addition of an equal quantity of hot water or of barley water. The total quantity of fluids required by the average townsman appears to be from two to three pints daily. We get rid of about two and a half pints of water from the kidneys daily, and about another pint of water from the skin and lungs, but we appear to manufacture water inside the economy by combining the hydrogen of the food with the oxygen of the inspired air (water = H_2O), but to what extent we do this has not been accurately determined.

CHAPTER IX.

On Heredity in Disease.

IT appears to me that the frequency with which disease is transmitted is greatly exaggerated in common estimation ; that is, I think that disease is comparatively seldom transmitted, while it seems, from what I hear, that the opinion is very generally held that it is often or very often transmitted. True, it is very difficult to get a clear idea as to what doctrine on this subject is generally favoured by the medical profession, and therefore by the public, who, on the whole, take their opinions on trust from them. But let us take a case or two from medical literature to shew what seems to be the common doctrine. A distinguished medical writer instances as a proof of the hereditary transmission of disease the following case. He has so little doubt, apparently, on the point, that he assumes, or seems to assume, that the mere statement of his case proves his theory of transmission. A lady of 45 years of age appears in his consulting room suffering from psoriasis, which is a dry, scaly, often reddish coloured or grey and reddish coloured form of skin affection. He is a good note taker, and has a good memory, and so

when, twenty or so years after, the daughter of that lady, now at the same age as her mother was twenty years before, appears in the same consulting room, suffering from an affection precisely similar, he is greatly struck, and turning to his notes and comparing the two sets of skin eruption, finds them to be identical, or almost identical. And, as I have said, the mere statement that the same or a similar skin affection has appeared in mother and daughter at similar ages seems to him (as to most other people also, if one may judge from the fact that no one has ventured to call the theory in question), to justify him in instancing the succession of cases as a clear proof of the hereditary transmission of disease. But is it? Where is the proof? Let me put my view of the case before we go further. I do not question the facts, which is a great comfort, since the issue between the two views is thus narrowed and limited, and much verbal discussion and disputation is avoided. I contend that organisation is transmitted, but disease not, or so rarely that we may as a practical theory or conclusion throw it out of account. Now let us look at the question for a little. Suppose that a man, and his father, and his grandfather have been soldiers, and that one after another at say 20 years' interval, they were each shot in action; should we expect anyone to agree with us in the suggestion that death by gun-shot wound or rifle bullet was hereditary in that family? And if the son of our last soldier became a grocer, are not the probabilities

great that he would die in his bed? It will be said, "Oh, that is quite different. Everyone can see that it was the circumstances of life, the occupation, the calling, the environment, which caused the three successive deaths in three different generations; but, in the cases of psoriasis quoted, there is no proof that this was so." It certainly seems very remarkable at the first blush that the psoriasis should appear in mother and daughter at the same age. I will now, however, state my theory in order that it may be seen side by side with the other, and (I suppose) the commoner view. Organisation is transmitted; disease not, or very seldom. Being translated, this means that: *like causes acting on like organisms in successive generations* (or in the same generation) *induce like effects.* Before I admit that the disease psoriasis was transmitted, I must know what the causes of psoriasis are (I know what the cause of being killed in battle, is) and then I must know (1) whether the mother was subjected to the action of those causes—whether, for example (for I think this is the chief cause), she took too many meals, too much bread, cakes, sweets, and puddings, but generally too much food; and (2) whether in her turn the daughter did the same. If they did, then I see no proof of the hereditary transmission of the disease, but I do see proof of my theory that too much food (essentially) too much bread, cakes, puddings, and sugar (accidentally) was the cause of the psoriasis first in the mother and then in the daughter. The

burden of proof of the hereditary transmission of disease is on the man who alleges its existence, not on him who objects to it. But I do not care about this. I am quite willing, so strong is my case, to assume the burden of proof. Take the case of consumption or of cancer. Consumption has been reduced in the British Army from 12 per 1000 per annum to 1·2 per 1000 per annum since the time of the Crimean war. The commission which sat upon the subject found that consumption was much more rife in the line regiments who had only 350 cubic feet of space allowed per man in barracks than among the guards who had 500 cubic feet of space per man allowed; so they recommended that the line regiments should be allowed 500 cubic feet of space per man also. But I never heard that they recommended that careful enquiry should be made into the family history of recruits for the army, and that those recruits should be rejected whose families gave a history of the presence of consumption. No doubt they insisted on chest measurements being taken, but that was done also before the question of how to prevent consumption in the army, or how to diminish it was discussed. A narrow chest is a condition of organisation, and it may be and is transmitted, but whether it will take on consumption is a question of environment. It is quite a question whether a man whose chest measures say only 28 inches in circumference, instead of 34 or 36 inches, ought to be admitted into the army at all.

The probabilites are that he will take on consumption if submitted to the hardships of a campaign, but that is a question of organisation and resistance. No doubt his resistance will be lower (other things being equal) than that of the fuller chested man. And if it had so happened, as very likely it might, that consumption was abundant in the narrow chested man's family, one may easily get a false impression regarding the transmission of disease, and may think that disease is transmitted, when in reality it is not disease which is transmitted, but the organisation, which, having a low resistance, is apt to take it on. So with cancer. Many families shew numerous cases of cancer occurring in successive generations. But cancer mainly is due to too many meals; and if the children of cancerous parents, eating four or five meals a day, would confine themselves to two meals, or, in rare cases, to one a day, there would soon be much less cancer to chronicle than there is now. Organisation is transmitted; not disease, or at least very rarely. But talking of cancer, or even of the psoriasis with which we started, raises another consideration, which appears to me to have received far too little examination. Is it not likely, if disease is transmitted, that it will appear early in life? It seems as if it should. If children inherit disease from their parents, should not inherited disease appear in infancy or childhood, since the nearer that children are to their parents the more prone they ought to

be to suffer from their parents' diseases? For my part I cannot get away from this conclusion; and so far as the evidence goes why should I wish to get away from it? Is not a clear conception of the facts the first thing we ought to aim at? and a sound theory to accouut for all the facts the next thing? It has, in fact, often seemed to me that perhaps the great mortality in the first year of life, and even in the first five years of life, may be partly due to a considerable amount of inheritance of disease. However, our medical officers of health appear to think, and I agree with them, that even here the chief cause of mortality is wrong feeding, and not inherited disease. Still, inherited disease may be a part of the cause of the high mortality of early life. Or we may put it: perhaps it is not even inherited disease which is the cause of the high infant mortality; it may be simply extra predisposition to it. I should like to say a word here about predisposition to disease. So far as I can see, predisposition is neither more nor less than weakness, or, as it may be better to say, it is inverse resistance. Now resistance is a quality of organic or organised matter (as it is, of course, of inorganic matter also), and if organisation is inherited, the qualities of organisation must be inherited also; and therefore predisposition to disease, that is, a less amount of resistance than usual to the causes, which, acting on the organism, and especially on the young organism, disorder it or produce disease (it is better to say, throw the

organism into disease, or cause the organism to become diseased), will be inherited also. It is well known that we all have very varying amounts of resistance, not only to causes which produce disease, but to influences outside and inside us, which produce all sorts of modifications in us. From this point of view we are all predisposed to every human characteristic; but we are not equally predisposed to each characteristic. To confine our consideration to the matter under discussion, we are all predisposed to the fevers, to inflammations, to rheumatism, gout, tubercle, cancer, and all other human diseases. Indeed, we may go further and say, we are predisposed not only to all these human diseases, but to the diseases of animal organisation in general; since the lower animals also suffer from all these diseases. So that, from our inheriting that general form of organisation which we share with animals (and even with plants), we inherit predisposition to all the disorders to which organisation is subject; while from inheriting the special form of organisation we call human, we are liable to every human disease and ailment. We are *liable* to all human ailments, but (except in a very few instances—I do not deny inheritance of disease—I have seen it, but it is very rare), it depends on how we manage ourselves, and in helpless early life it depends on how our parents or guardians manage us, whether we shall be *subject* to diseases or not. To say that this, that, or the other disease is not known in our family, or that a

given disease is particularly rife in our family, is only to say that the influences making for its production were not or were great among the members of our family on the one hand; or on the other, that our family's resistance to these influences was or was not great, or was or was not greater than that of other families. In examinations for life insurance many companies ask a question like this: is your family particularly prone to any form of inherited disease, to rheumatism, gout, cancer, or tubercle, or other disease known (?) to be hereditary? And the answer is generally: none of these diseases are known in my family; but in point of fact all diseases are known in all families, but in some more and in some less. It is not only in mental and moral regions that one may say *Homo sum et nihil humanum alienum a me puto;* but this is chiefly because like causes acting on like organisms in succeeding generations induce like effects. Predisposition is therefore inverse resistance. It seems a mere play upon words to say that no one can suffer from any disease who had not the predisposition to do so; and yet a great writer has delivered himself of this truism as regards the acquisition of physical characteristics. If the resistance of the organism to the poison of influenza (or to any other influence) is great, then predisposition is small or slight. If, on the other hand, predisposition is strong, then resistance is slight. Predisposition and resistance seem to be inversely as one another, the greater the

one the less the other, and the less the one the greater the other. If health is maintained by proper management in respect particularly of proper relations between the body and air, food and exercises, then the economy will be able to oxidise off and to resist without illness a much larger amount of the poison of influenza than if the air we breathe is foul, the food we consume is unsuitable, either in amount or quality, and the exercises or movements we perform are insufficient or not properly methodised. If health is at a standard sufficiently high as regards these conditions, it seems as if the poison of influenza would or might have no effect on us at all. Of course, any one who has been compelled by reflection and evidence to adopt this view regarding the susceptibility to influenza or other infectious diseases, would not hesitate, if he were himself attacked, to admit that he felt somewhat humiliated, since he would feel that he had not been able to live up to his own principles. If, on the other hand, he were attacked by influenza with complications, like pneumonia, or the long depressed condition which sometimes follow it, ending in consumption or organic heart disease, he might even feel disgraced, since he would know that in both of these conditions he had been improperly fed, and, especially in the case of influenza occurring with complications, he had been for a long time previously to getting it, over-fed. I might like to stipulate for the absence of very severe and

very long continued anxiety as a predisposing cause in this case; but the amount of anxiety to which I refer would have to be very great indeed, such, for instance, as befel Job when messengers announced to him in quick succession the loss of his oxen, and asses, and the servants who were tending them, his camels and their attendants, nay the destruction even of his dwelling-house besides; or, as has occurred more than once in modern business life, when a man has lost goods, and credit, and friends, and when even his family have not escaped from sickness and death. And if, when a man's health suffered from these causes we could conceive it possible that even his wife should turn against him, his anxieties would be so great as probably to make him ill, however properly he had managed his air, food, and exercises. But such combinations of misfortune are rare indeed, and are comparable rather with the effect of volcanic eruptions, or earthquakes, or tidal waves, which, though they do occur sometimes in the physical history of our earth, are so rare as to account for only a small part of the evils from which, on the whole, we suffer. By establishing proper relations between the body and air, food, and exercises, but particularly between the body and food, we may get, it seems to me, into such a condition as not to fear the pestilence that stalketh in darkess, nor the destruction that wasteth at noon-day. Neither we nor our households need have any terror or fear of them. And the state of freedom from the

apprehension of infection to which one person or one household may attain, may be attained by all. It is not necessarily the strongest persons, but often the most delicate; it is not those whose original predisposition or inverse resistance or weakness is least, but often it is rather those whose predisposition and weakness is greatest who seem most easily influenced by the kind of arguments here advanced to aim at the practices of self-restraint and self-government here suggested, with the freedom from care, and anxiety, and disease, which these practices bring in their train. I do no more here than make the barest reference to the moral implicates of these suggestions, or the bearings they have on life in all its manifold aspects. These may perhaps be elaborated and amplified by others more competent to deal with them. I confine myself to pointing out the bearing they have, if correct, on the avoidance of suffering, on the number of early deaths their carrying out might prevent, on the diminution of the number of widows, widowers, and orphans they might effect, and on the clearing from our minds of those unjust aspersions, and of the cant and self-complacency which blames our ancestors or our fate for evils which, to a great extent, we inflict on ourselves.

Intimately connected with the term predisposition are three other terms frequently used by medical writers and speakers, viz., constitution, diathesis, and heredity or atavism. To these we ought to devote a little consideration. I

propose to use these words in senses perhaps a little different from those in which they are commonly used—as commonly used, they are often convertible or synonymous with one another—but I do so because it is well to limit the sense in which words are used so as to attain clearness of ideas regarding them, and also because in the present case there are three natural divisions of life to which the three words in question can be easily confined, so that this desirable clearness can be attained. By *constitution,* then, I mean to denote the state of the human organism from the moment of birth to death. Constitution, from this standpoint, being the resultant at any and every moment of the interaction between the organism or economy and its environment, that is (mainly) between the economy and its air, food and exercises; it follows that the constitution can be, and is, slowly changed as the relations of the organism to these three things (mainly) or generally as its relations to its environment alter and change. The environment is the sum of the circumstances affecting the organism from birth till the moment under consideration. A complete account of the environment would be a complete history of the relations between the organism and its surroundings from birth till the moment under consideration; but while it is evident that an account or history so complete as that is unattainable, the general effect of the surroundings upon an organism can be observed well enough, and the influence of the more important of them can also be

observed. But constitution, it is evident from these considerations, must be always flowing, mobile, changing, as the circumstances of the environment change, flow and alter. Every action or experience in the body must have its influence on the constitution, which is incessantly changing; and, as any given body is capable only of a certain number of changes before it ends or dies, each action must have its effect in hastening that termination. But it is not possible to estimate the effect of one action, and still less to measure it with any approximation to accuracy. All we know is that its influence must be incorporated in the constitution. But the effects of long-continued mal-assimilation are plain enough, not only theoretically but practically, and their influence in hastening the termination of life is often quite clear. Ailments, small and great, make their appearance, as we have seen, and very few constitutions come to an end from simple old age.

I think perhaps I ought to draw attention here to the influence we may exert in modifying or altering our environment. It seems to me as if not enough attention was generally given to this point. We seem to assume too readily that environment is a set of conditions outside of our power and influence, as if environment influenced us rather than we it. But both are true. Environment acts and reacts on us, no doubt; but we in turn act and react on it. If it is true, within certain limits, that environment makes us, it is also

true that we may make and that we do make our environment. In the moral sphere it depends greatly what use we turn our trials to, what effect they have upon us; but I do not speak of that. It is true also in the lower physical plane; and by the proper and moderate use of the great natural environing factors of air, food and exercises, the quantities and relations of which are very greatly in our own power, we can modify for good, and the building up of a healthy resistance to all forms of disease, the organism which we inherit and possess.

Heredity or *atavism* is the state of the organism as determined by the ancestors. Whatever this may be (and a very complicated subject and complicated condition it evidently is), it is plain that, unlike constitution, which is always changing, heredity is fixed and determined. Our ancestors have come and gone. They have lived their lives, and transmitted their organisation, with its qualities, whatever they are. Our heredity or atavism, transmitted by them (mingled with the *diathesis* immediately to be defined), and from them received by us, is fixed and determined. What it was and is, that it remains. Nothing can alter it. The heredity of a human being determines humanity. That of an ox bovinity. That of a fox vulpinity. That of a dog caninity, &c. A human being, an ox, a dog, or a fox, however, will live healthily or otherwise, rather in accordance with the circumstances of their respective environments than in regard of the state of their ancestors; rather in

regard of their own changing constitution than of their heredity. But, while the heredity in general of human beings determines humanity, the heredity in special of a particular human being determines the special hereditary characteristics of that human being. As, however, these hereditary characteristics, or characteristics received through inheritance, are determined by ancestors who have come and gone, it is evident that their influence, whatever it is, is fixed and determined, and cannot be altered. Such characteristics as size, colour of hair and eyes, and even gait and handwriting, dependent on shape and form of hands and of the nervous mechanism through which it is governed, are all hereditary characteristics, and maintain themselves, as we see, from generation to generation. And the evolution of the changing, mobile, fluctuating constitution through the fixed and now changeless heredity is like the moral relations of life, which may be described as the play of the temporal on the eternal, as the movement of that which is for a day on that which is for ever, an analogy which I do not elaborate any further.

Lastly, there is a third condition or period of history in the life of the organism, namely, its intra-uterine history. To this it is proposed to confine the term or name, *diathesis*. It is evident, if this definition or limitation be accepted, that diathesis is a state intermediate between heredity on the one hand and constitution on the other; and further, that diathesis, like heredity, but unlike

constitution, is fixed and determined. If it be objected that the proposal to limit these three commonly used words to these particular senses is somewhat arbitary, the reply must be: there are three great natural periods in the history of organic beings, and these three words seem naturally to fit, or can be made to fit, or suit, each word one of the natural periods. And whoever is unwilling to accept these definitions ought to be prepared with the suggestion of better ones.

With these ideas in our minds, it may be well to try to see further into what is transmitted or inherited. A very ancient writer delivered himself of the following views. "If, then," he says, "bald persons arise from bald persons, and blue-eyed persons from blue-eyed persons, and squint-eyed persons from squint-eyed persons as a rule, and as the same rule holds good as regards other forms, why may not long-headed persons be produced from long-headed ones?" And again, speaking of epilepsy, which the priests had called the sacred disease, contending that if they cured it they must be considered the representatives on earth of divine power, while if they did not cure it, who could, they said, wonder that they failed since it was a sacred disease? contending against and opposing this doctrine, he says: "For if from phlegmy persons phlegmy arise, and from bilious persons bilious are generated, and from phthisical persons phthisical, and from spleeny persons spleeny" (those who suffer from affections of the

spleen, a blood-making gland), "why may not those parents who suffer from epilepsy transmit it likewise to their descendants?" These passages are very interesting, as shewing the ancient view of the heredity of disease. It appears to me that they mix up in the most delightful and the most unconscious and confusing way the transmission of organisation and the transmission of disease. We should admit to-day that blue-eyed persons have as a rule blue-eyed children, and (physically) long-headed persons long-headed children. We might or might not admit that bald persons have bald children. In fact most children are born with hair, which they soon lose as a rule, becoming quite bald; after which the hair grows again, fair in the case of those descended from fair-haired ancestors, and dark in the case of those descended from dark-haired ancestors. And we might or might not admit that squints were transmitted; while we should probably think that, not epilepsy, but the unstable organisation of nervous system, which easily takes it on, was transmitted—at least this would be my view. But we should all (I suppose) deny that phlegms or catarrhs were generally transmitted, that biliousness comes down from ancestors, that consumption is transmitted, and affections of the spleen. At least I do not know, but I suppose we should. It is very difficult to get at the mind of the medical profession on this subject, if, indeed, it has made up its mind. Some time ago I was treating as more or

less ridiculous the idea that special susceptibility to take fevers ran in families, or contending at any rate that only resistance as a quality of organisation was transmitted; when a distinguished physician wrote to say he did not at all agree with me, and that he felt convinced that some families shewed much more predisposition to take fevers than did others. If this means anything more than that resistance is greater or less in different persons, and in different families, to the causes which induce fevers, I confess I do not know what it does mean; but in that sense I quite agree with him. As, however, he combated my view, I suppose he must have meant something more. What I meant then, however, and mean now, is this. I must admit, of course, that varying degrees of resistance to the causes which induce epidemic and other disease, are transmitted. This is a commonplace of observation, and is only to say, what everyone knows, that infants come into the world with varying degrees of strength. But, every minute that life lasts, we begin to alter our original strength or weakness, or our predispostion, by the ways in which we live, or in which our parents or guardians make us live; by the care or carelessness, or the wisdom or folly with which they treat us. And so it comes about that in a comparatively short time we alter, or have altered for us, and that to a very considerable extent, the predisposition to disease, or otherwise the weakness of resistance against it with which we started life. And obviously this is more and

more our own doing as we go on in life to form our own habits, and to gradually go on into delicacy or strength of constitution, and manifest (what we really therefore make for ourselves), more or less readiness to be attacked by epidemic or other disease. This is only an illustration of what was said before, that constitution is always flowing, mobile, changing, coincidently with changes in the circumstances or environment to which we are subjected, or to which we subject ourselves.

A phrase, by the way, which our ancient authority makes use of, and that twice, once in each of the passages which I have translated from him—ὃ γὰρ γόνος πανταχόθεν ἔρχεται—reminds us very much, or rather may be said to anticipate, in a somewhat remarkable manner, Charles Darwin's theory of pan-genesis, especially as in his second statement he adds the words τοῦ σώματος; and we seem to see the organic body throwing off its gemmules from all parts.

Discussion of such a subject as this might easily fill a treatise; so I will only go on to say briefly one or two words more about it. The view I am advocating seems to me on the whole to take more and more of the influences that determine the course of life out of the hands, so to say, of fate or necessity, and to shew more and more how much we have in our own power. I did not adopt it because of this feature at all; but having come to it for other reasons, I found that it did lend support to the more hopeful view. I once saw the liver of

a child of eight years of age, which shewed all the features of gin drinkers' liver, of what is known as the hob-nail liver, although the child had never tasted gin at all; but his father was a drunkard. An appalling fact of this kind fills the mind with awe, as we seem to see the sins of the father visited on the child; and it is facts of this kind which prevent one from denying that disease is sometimes transmitted. But how rare this is after all. In thirty years no other case like it has come under my observation. No doubt at all, if one's eyes had been opened, more such cases might have been observed; but even then I believe they would have been few, and certainly the evidence does not warrant any other conclusion than this, that the vast majority of the children of diseased parents, even of drunkards, are born healthy. This view is in keeping with the latest expert opinion on the subject, that acquired characteristics are not transmitted, that is, that they are not often transmitted, although it is equally plain that they are transmitted sometimes; indeed, if they were not, I do not see how evolution could be possible. But that it is not the rule that they are transmitted, but quite the contrary, is shewn not only by direct observation, as we have seen, but also from this theoretical reflection that, if they were, the race must long before now have been swept away from off the face of the earth, since not only we, but all our fathers, have broken, and that not once, but repeatedly, the laws of life and health.

It may be said that it is not always possible to distinguish between the organisation which, I say, is, as a rule, transmitted, and the disease, which, I contend, is not or not often. This is true, no doubt. Organisation and disease shade off into one another by a large variety of gradations, so subtle that it is very difficult, if not impossible, to say where organisation leaves off and disease begins. A defective structure of the heart valves, for instance, would be an example of imperfect organisation, and might be transmitted. If it were, the moment that such valves began to act, it might be quite impossible to distinguish between the defective organisation which caused imperfect circulation of the blood, and the nutritional signs of that imperfect circulation. But this is a kind of difficulty which, as we before saw, is continually being met with in our study of nature; and we have seen instances of it before in the attempt to separate day from night, heat from cold, and, in fact, all contraries from one another. The poet, indeed, revels in the illustration of such subtle gradations, and much poetry and a good deal of philosophy turns on it. The idea is well illustrated in such a quotation as that

> "Day in a breathless passion kissed dark Night,
> And neither spoke. And in that kiss Day died."

The idea would have been equally well expressed if night had been made to kiss the day, or the hot the cold, or the dry the moist, or any other opposite its contrary; and yet this does not prevent us

from being able to make practically workable demarcations of day from night, of hot from cold, of dry from moist, nor of organisation and its healthy functioning from diseased functioning. A good idea of what is transmitted and what is not, might, it has frequently seemed to me, be obtained from the attempt to answer the question : how is it that every baby born into the world comes in with the soles of its feet a great deal thicker than the tops or dorsa of its feet, and yet is born without corns? If a long succession of walking ancestors, each using in paws and feet the under parts of the feet to walk on—if this long succession of walking ancestors determined the former, why not the latter also? And the only answer that occurs to me is that it seems to be because the long line of walking ancestors, whose habits brought about the former, was a much longer procession than of the ancestors whose feet were pressed in tight boots and shoes. But if this is the correct answer, then it is evident that characteristics acquired in an indefinite number of previous generations must in course of long ages be transmitted; but in what generation the characteristic in question began to be transmitted, it is exceedingly difficult and even impossible to say.

Another question arising out of the views advanced here is this. We can see it arising in the cases of psoriasis with which we started, and in the attempt to answer the question whether cancer is transmitted. If the mother and daughter who had

the psoriasis were each of them 45 years of age before they shewed the psoriasis, and if the mother was 25 years of age when her daughter was born, is it likely that the mother transmitted to the daughter a disease which she did not herself have till 20 years after her daughter was born? This does not appear to me likely, I must say. Or take the case of a mother and a daughter, both having cancer say at 55 years of age. A precisely similar question arises. If the mother was 25 at her daughter's birth, is it likely that she should have transmitted to the daughter a disease which she did not herself manifest until 30 years after her daughter's birth? This does not seem likely. But if, on the other hand, we can shew that improper feeding, or any other breach of the laws of healthy life, caused the psoriasis or the cancer in the mother, and that, in the course of 20 or 30 years, similar improper methods of feeding (or any other causes) caused psoriasis or cancer in the daughter, then we have efficient causes to account for the facts without having recourse to the suggestion of hereditary transmission to account for it. We have to answer the question: whether is it more likely that something done or left undone by the mothers 20 or 30 years or more before the daughters were born is the cause of the psoriasis or of the cancer, or whether is it more likely that something caused these diseases which the daughters themselves did between their birth and the 45 or 55 years which elapsed before the psoriasis or the cancer shewed

itself? As a question of probability, surely no one would deny that the latter is far more likely, as it also seems to me to be that if the diseases were really transmitted they would probably have appeared in early childhood, when the influences of the parents on their children must have been much stronger than we can suppose them to have been at a later date. This is not a mere academic question. It has the most important practical bearings. We are continually being pressed with questions and suggestions of this sort. Children whose parents are suffering or who have died of what is believed to be hereditary disease, come from time to time to their medical adviser to ask his opinion on this question: Am I likely or certain to suffer as my mother or my father has suffered? Or they are more or less depressed by the suggestion that they are more or less certain to suffer. If the medical adviser can conscientiously reassure them on such an important issue, much will be gained. If he can say: You have your mother's organisation, no doubt, but it depends on how you manage that organisation whether you in turn suffer from her disease; the affair is in your own hands; avoid the causes which made her ill, and you need not suffer—if the medical adviser can say this, as it seems to me he can, what a load is taken off the daughter's mind or the son's mind; and what a load is also taken off the mind of the medical adviser. The mere removal of the apprehension may be, and I believe often is, a contributory cause in preventing

the onset of the disease which the child fears; and although I did not adopt my view of heredity in disease in order to be a suggester of hope rather than a prophet of evil, I feel very glad indeed that the opinion to which I have come from the evidence (as I think and believe) carries with it this suggestion of hopefulness, as it carries with it the advice that if we avoid the causes of our ancestors' diseases, we shall most probably not suffer from them. Of course, we must wear out and we must die of something, but we ought to die worn out, and of old age, not of disease. And, at least, it makes the most tremendous difference to our families whether we die, say at 55, or live to 70, since in the latter case we have the opportunity allowed us to help them in their entrance upon life, and in the former we disappear just about the time when our presence and influence would be of most use.

CHAPTER X.

On the Feeding of Children.

THE inquiry into the heredity of disease, resulting, as it has done, in the general conclusions that it is organisation which is inherited rather than disease, and that the general law of heredity is that like causes acting on like organisms in succeeding generations induce like effects—that inquiry seems naturally to lead up to some observations on the feeding and management of children. These observations need not be numerous, because the principles which apply to the feeding of adults apply also to children, and may be summed up in the one word moderation. Too frequent and too abundant feeding unfortunately obtains in the case of children as well as in the case of grown persons. Before I deal with this, however, and with what the reader has probably mentally observed to herself—how can the principles be the same when the child is growing, while the adult is fully grown ?—I should like to say just a word or two on another point. I have several times said that I thought that anxiety must be reckoned among the predisposing causes of illness. Well, in the case of children, this cause is almost entirely absent. If

children are destitute (in a sense all children are destitute, being paupers, as it were, and dependent on the love and charity either of their own parents or relatives, or on that of strangers), they are taken charge of almost always by some one, and seem to accept the inevitable in their lot in the most matter of fact way, without murmuring, or repining, and with little of that anxiety which is one of the causes of illness, and of the aggravation of illness among grown people. This cause being, therefore, almost wholly eliminated, the problem of managing children's ailments, and of maintaining them in health, is by so much more easy and hopeful. But in them, as among adults, one of the chief, if not the chief cause of illness is also errors of diet. And, more than this, I believe that the chief cause (after making what allowance is necessary for hereditary disease—a larger amount probably than obtains in the case of adults), of disease among children is not only errors of diet, but that the particular form which those errors of diet take is too frequent and too abundant feeding. From the cradle to the grave this cause seems to afflict us. Even the poor suffer from it, for what is commoner among all classes of the population than to feed infants too frequently, every two hours for example, or every time they cry; and how common are digestive ailments in consequence of this, as shewn by the presence of flatulence, colicky pain, constipation or diarrhœa; or the occurrence of some of the continued fevers; or of bronchitis or

months, 6 ounces; at twelve months, 9 ounces; and at eighteen months, 12 ounces; but in early life the digestion effected in the stomach is almost *nil*, the stomach acting much more as a reservoir than a digestive organ, and passing its contents rapidly through into the intestine, where practically all the digestion has to be effected. The number of feeds necessary for infants varies very much according to the different views of the different experts, and it is very interesting to compare different practices with one another. In ancient times it seems to have been the practice to feed much seldomer than now. For instance, Oribasius (B.C. 403—326) says: "*Puer nuper in lucem editus, melle primum nutriatur*" (is this the source or authority for the custom followed yet by so many old women of thrusting a piece of butter and sugar into the mouth of the newly born child?) "*deinde lacte bis in die vel ad summum ter.*" Milk twice a day or three times at the outside, that was Oribasius's prescription for the feeding of infants. And Paulus Aegineta, about 1000 years afterwards (Circa 625 A.D.), almost slavishly follows him, for he says: "*Primum alimentum recenter nato infanti mel exhibere oportet; postea vero lac praebere bis in die aut ad summum ter. . . . Sufficit autem biennium lacte nutrire; deinde ad cibos transgredi.*" The infant was to be fed with milk twice a day or thrice at the outside, and the milk was to be continued for two years, at the end of which time a change was to be made to other foods. How different and how much better a

plan was this than the modern habit of feeding babies every hour and a half or every two hours, or than that which obtains so much of filling them up with starchy foods which their digestive apparatus is unable to cope with. No wonder that from 160 to 200 infants should perish in the first year out of every 1000 born. Even if we think that two or three meals a day are too few for infants, four are in most cases quite enough, say about 8 a.m., 12 noon, and 4 and 8 p.m., with one feed in the night. And no starchy food at all is required or should be given for at least the first nine months of life, after which it should be given only once a day, milk being used at the other diets.

I think I ought now to say something about the growth of children in reference to feeding. The fact that a child is growing makes, no doubt, an important difference in comparing its feeding problem with that of a full grown adult. But to how much does this amount? A baby weighing say 7 lbs. at birth loses, as a rule, nearly a pound in weight in the first three days, after which it gains on the whole steadily (though sometimes in the intermittent way which, as we have more than once seen, characterises the movement of organic phenomena on this planet), till, in twenty days, it will weigh nearly 8 lbs.; at the end of a month about $8\frac{1}{2}$ lbs.; at the end of two months about $10\frac{1}{2}$ lbs.; at the end of three months about $12\frac{1}{2}$ lbs.; at the end of six months about 16 lbs.; and at the end of the first year of life about 20 lbs. In one

sense the gain is immense, because a baby weighs at the end of a year nearly three times its weight at birth, and this must no doubt mean great developmental activity in every part of the growing body. I dare say that this great nutritional activity and the changes it implies form a main part of the cause of the great mortality experienced in the first year of life; and probably to this must be added some amount of hereditary weakness or occasionally, inherited disease. But in another sense the gain in weight from 7 to 20 lbs. is only a gain of 13 lbs. in a year. This again is about a pound a month, or about half an ounce a day on the average. We may, however, suppose that some overplus of food ought to be ingested into the growing body, over and above absolute necessity, and we may, therefore, in response to this demand (a reasonable one I think), say that we ought to supply as much food as will afford one ounce a-day or even one and half ounce in addition to that required for subsistence. I do not know how this will strike the reader, but I think she will say that the requirement is a good deal less than she expected it to be. Considering the composition of milk and of body-tissues respectively, this demand for growth amounts to say three or four ounces of milk daily in addition to what is required for subsistence. What I suggest is, that, under the demand for food for increase of growth, a demand vague, and because vague, usually much in excess of requirements, a great deal more food is given to babies, in order to meet this growth-demand,

than the three or four ounces of extra milk which they require for this purpose. The consequence of this is a great deal of mal-assimilation of food or of indigestion, shewing itself in flatulence, pain, griping, constipation or diarrhœa, or in disturbed and irregular sleep, and in various "colds," and tongue—mouth—or throat—troubles. Mothers, and especially young mothers, are so anxious to do well by their babies, that they do too much for them; they overdo, and the children suffer. Of course children are sometimes neglected, but this is a much rarer form of error than the other; albeit that it shocks us very much in those comparatively rare cases in which it occurs. Perhaps a mixture of neglect and of over-indulgence is the commonest form of error in the treatment of infants, too long intervals between meals at one period of time being attempted to be made up by too frequent feedings at another. But this form of error is often fatal; and it really would be better if, by any chance, baby, who ought to have been fed in four hours, has had to wait six, to give it its food then, and to wait the full four hours next time, than to feed say in two hours again, or even in three, in order to make up for the previous omission. Clear ideas, then, regarding growth, its rate, and its amount, would vastly help us in managing the nutrition of infants, and would tend to prevent the over-feeding and the too frequent feeding which are so common a cause of illness and of mortality among them. While I am considering this subject,

I should like to add some similar considerations regarding the nutrition of growing children, say at ages beyond the first year. A growing boy or girl, we are constantly hearing, ought to be well fed. Yes; so they ought; but *how* well ought they to be fed? That is the question. Comparatively few observations have been published upon the weight of children from the second to the fifth year of life. But from 372 observations made by Professor Holt, of New York, it appears that the gain in weight is about six pounds during the second year, about four and a half during the third year, and about four pounds during the fourth year. During this period the gain is rarely steady even in the second year. With most children it is slowest, or the weight is stationary, in the summer months, while the most rapid increase is usually seen in the autumn. Throughout this period girls gain in about the same ratio as boys, but remain on the average nearly one pound lighter. "During almost every illness, no matter of what character," says Professor Holt, "the gain in weight ceases, and usually there is a loss, the rapidity and extent of which are somewhat proportionate to the severity of the attack; but it is always much more rapid in diseases of the digestive tract than in any other form of illness." The reader who has entered into the spirit of the observations characterising this essay, will have no difficulty in seeing how these remarks coincide with it, or in perceiving that I look upon the rapid loss of weight in children's illnesses as generally caused

by the too rapid accumulation in their blood and tissues of ill-assimilated food stuff, converted into ill-made body stuff. This ill-made body stuff is apt on exposure to a variety of the ills of life, to cold, wetness, wind, damp, or fatigue, which, do as we will, cannot always be prevented, to die, break down with excessive combustion or fever, and require to be eliminated from the body. If we translate this statement into the language with which we are now familiar, we may say, that exposure of infants to the exciting causes of cold, wet, damp, fatigue, &c., is apt to have very hurtful influences on their health. but that these hurtful influences are apt to be much aggravated if in addition they have been subjected to the action of the predisposing cause of wrong feeding, and, as is so commonly the case, of over-feeding, that is, of too frequent or too abundant feeding. The principle, therefore, which determines the occurrence of the large majority of the diseases from which infants and children suffer is just the same as that which causes their occurrence in grown persons, viz., wrong feeding, the considerations advanced regarding normal increase of weight in infants exactly and strongly corroborating this view. Even the beggar's baby, as has been already said, is under the same influences; too frequent feeding, and that generally with improper sorts of food (particularly starchy foods) being the chief cause of its illnesses.

As regards the increase of weight beyond the age of five years, the following table from

Bowditch, referring to American children, will shew at a glance the facts. The height and circumference of the head and chest increase proportionately to the increase in weight, but I have not inserted them in the table.

Weight of Children from Five to Sixteen Years of Age.

		Boys.			Girls.	
Age.		Pounds.	Kilos.		Pounds.	Kilos.
5 years	...	41·2	18·71	...	39·8	18·06
6 ,,	...	45·1	20·48	...	43·8	19·87
7 ,,	...	49·5	22·44	...	48	21·78
8 ,,	...	54·5	24·70	...	52·9	24·01
9 ,,	...	60·0	26·58	...	57·5	26·10
10 ,,	...	66·6	30·22	...	64·1	29·07
11 ,,	...	72·4	32·83	...	70·3	31·87
12 ,,	...	79·8	35·21	...	81·4	36·90
13 ,,	...	88·3	40·04	...	91·2	41·36
14 ,,	...	99·3	45·03	...	100·3	45·50
15 ,,	...	110·8	50·26	...	108·4	49·17
16 ,,	...	123·7	56·09	...	113·0	51·24

The slowest gain is from the fifth to the eighth year, when it is about four pounds a year. From the eighth to the eleventh year it rises to about six pounds a year. Up to the eleventh year the two sexes gain in about the same ratio. From the eleventh to the thirteenth year the girls gain much more rapidly, passing the boys for the first time, and maintaining this lead until the fifteenth year, when again the boys pass them. But the rate of increase for boys and girls alike emphasises the truth of the considerations I have advanced regarding the need for feeding for growth, and

the comparatively small amount of extra food required on this account. As a fact, growing boys and girls are frequently supplied, not with half an ounce of extra food daily to allow them to grow, nor with an ounce, or even two, three or four ounces, but, on the ground that they are growing, are not infrequently provided with an extra half pound of food three times a day; and, in consequence, most disastrous results not infrequently occur.

I might say here, in a parenthesis, as it were, that exactly the same principles determine or ought to determine the kind of advice that ought to be offered to the expectant mother, so often advised and even besought by her friends to "eat for two." As in nine months there is brought forth a product weighing altogether about nine pounds, the problem of properly feeding her is very similar to that of feeding the growing boy or girl, and amounts to an extra quantity of between half an ounce and an ounce a day. Even if the gross weight at birth were 18 pounds (and it occasionally approximates to this weight), the extra necessity of food-supply would only amount to about an ounce a day. But the vaguest ideas being current as to this necessity, well-meaning but injudicious friends press upon her the advice to perhaps interpolate an extra meal or even two in the day, and to increase her normal food supplies besides, so that in order to meet a necessity of an extra supply of an ounce, or say two ounces, of food daily, perhaps an extra one and

a half pounds avoirdupois may be ingested and consumed. The consequences of this only those know or can appreciate whose business it is to bear on their shoulders the responsibility for disasters occurring at parturition — disasters of the most calamitous kind, which always evoke, as they ought to do, the widest spread sympathy, too often accompanied by execrations of nurse and doctor, who did or did not do this, that or the other trifle, the doing or not doing of which led, in common opinion, to the calamitous occurrence. It is not the lighting or not lighting of the fire at a given moment, nor is it the opening or not opening of a window or of a door, causing a draft on the one hand or too much stuffiness on the other; nor yet is it the bringing of a certain meal five minutes after its proper time—it is not any of these trifles which does the mischief, although all things ought to be done, no doubt, decently and in order; but it is the improper habits of the patient herself, and particularly the unsuitability of her food habits, before her confinement, which is the main cause of her disasters. The exciting causes named may indeed be the occasion of the calamity—they may be a part or a small part of the cause; but *the* cause, or the main part of it, without whose existence the slight or exciting causes would have been powerless for evil, is the state of the patient herself, brought about by the action of causes so simple as to be within the observing power of the meanest intellect, and yet, perhaps for

that very reason, overlooked or ignored by almost all.

If we keep in mind the principles which ought to guide us, we shall have little difficulty in answering the question so often put to us, how often do children require to be fed? I have already dealt with infancy. After two years of age a child ought to be got on to three meals daily without any interpolation of extra meals, without addition of chocolates, spice, cakes, &c., if we wish it to be well, and to exemplify and maintain those characteristics which seem to make the period of from two to four years of age the very acme and perfection of childhood. Nothing probably is more interesting and attractive in the whole range of human experience than to watch the opening of childhood's faculties at this age, to hear the early attempts at connected speech, as nerve cells in the brain become connected with one another so as to form the mechanism through which the growing powers of mind can be better and better exercised. Equally interesting is it to watch the development of the moral and spiritual and the emotional nature, as by leaps and bounds in an intermittent, but, on the whole, progressive way, it opens up before us; similar changes in the posterior parts of the brain taking place, so as to allow of the manifestation of these qualities, and of remembering, judging, &c., as took place in the anterior portions to allow of feeling and perception. But if these faculties are to grow and develope from early promise to maturer

fulfilment, how important it is that from the earliest age the necessity of self-government should be put before the child, how necessary that it should be taught that, while learning its needs from the impulses of nature and of desire, it should early learn to put restraint and government upon them. And until this is possible, how necessary that parents and guardians, who stand to children in the same relation as divine providence to children of a larger growth, how necessary for them to make the conditions as suitable, and natural, and healthy as possible. The same considerations obtain and ought to weigh with us in feeding the growing and developing cerebral organism as obtain in the case of the nutrition of older persons; only this is, if possible, more important in the case of children so delicate and so tender. If, for example, they have a great love of sweets, this should be gratified—it was not implanted in them by nature for nothing—but it should be gratified in moderation, say by giving one or two of the coveted edibles at meal times, and not between meals, so that they shall be digested and properly assimilated with the food, and not go to form subidiary meals. For, if they do the last, how can the blood fail to be loaded with waste and effete, or, at least, with ill-made and imperfectly assimilated food stuffs? And when this is so, how can we expect that the blood will either nourish the body properly or secrete the fine spirituous fluid lying in the ventricles of the brain, and between the membranes of the brain and

spinal cord, so that the functions of these governing and controlling parts of the body shall not fail of their high place and power? Will not that fine spirituous fluid tend in such circumstances to become thick, cloudy, and grumous, and so to spoil the development and growth of those very faculties whose proper growth is of so great consequence to their possessors, and of interest so intense to on-lookers? That there must be something wrong in the way in which children are generally fed will be apparent to whoever calls to mind and reflects upon her experience on seeing her child in the early morning, and on discovering from the morning kiss how unpleasant (not to use a stronger term) is the child's breath. It ought not to be so, but the breath is too often faint and mal-odorous in place of sweet and fragrant. This must mean mal-assimilation of food, and mal-assimilation of food must, in turn, result in imperfect formation of bodily and cerebral tissue, in other words, in improper growth of the child's tender body and brain, and in marked instability of tissue, which is therefore far proner than it ought to be to break down with excess of combustion and oxidation, and to induce one of those inflammatory or febrile attacks which are so characteristic of childhood.

I will not say any more about the interest and even fascination which characterise the growth of body and mind in early life; nor will I say anything more about the gravity of the responsibility undertaken by parents in the rearing and education

of their children. Truly, when we come to reflect upon it, we are driven to exclaim: "who is sufficient for these things?" But I will, coming down again to the lower physical and organic basis on which the higher qualities are founded, and through which they work, consider a little further the number of meals proper to childhood, and how best to meet the indications of physiology and of nature in the circumstances of modern civilisation in towns. I do hope I may carry medical opinion with me in the view I am enunciating. I do know that some able medical authorities agree with me in thinking that three meals a day are sufficient for children in all circumstances, and that in no case, therefore, should the practice be resorted to of providing them with more. If, for instance, children of from five to fifteen years of age breakfast at eight, go to school at nine, and dine at 12-30 or one, they should in my view not be encouraged or even allowed to eat anything at the ten minutes' break during school hours at 11 p.m. At the most, they might have a glass of milk and hot water, equal parts, at that time, although I really think a glass of water alone would be better for them, since it would stimulate the stomach, and the digestive organs generally, to finish the digestion of breakfast before dinner was due. The digestive organs would thus have more rest, the blood-making processes would be better performed, and fewer causes of "colds" and feverish attacks would be present in the body and

blood; while dinner at its proper time would be better enjoyed and better assimilated. As to the habit, not uncommonly indulged in, of taking tarts and milk at eleven o'clock, it is a pernicious custom, which I am quite sure ought to be abolished. It leads to many colds and many illnesses, fevers among the rest, the causes of which are apt, as we have already often seen, to be attributed to cold, wetness, fatigue, &c., on the one hand, and to that bugbear of modern life, "infection," on the other. If only parents and the public generally could be induced to see that when small amounts of exposure to cold, rain, wind, and the like, give children "colds," and that when small and often scarcely discoverable amounts of exposure to infection give them "fevers," the predisposing conditions as to feeding ought to be carefully considered and examined, how much benefit might be obtained. Sometimes, it seems to me, the "infection" is boldly invented, when a "fever" has to be accounted for. A child, for instance, is, say, six months in hospital when it developes scarlatina or diphtheria. At once infection is looked for. A visitor, it is said, must have brought the infection. This is assumed as an unquestionable axiom. Careful inquiry is made. Someone who visited the child is proved, or is suspected to have been in communication with someone who has had scarlatina or diphtheria, and in this obscure and subtle way the infection, it is inferred, has been introduced into the ward. The

idea that the child might have developed the fever because say a germ or two of the infecting cause was swallowed in milk or eaten in food, and, finding in the over-fed body a suitable nidus or soil, took to growing in the blood or tissues, is never considered. Nor is it considered, as it ought to be, that if a cause so slight as the vague exposure to infection on the one hand, or the introduction into the organism of a germ or two on the other, suffices to give a child a fever, *the* cause, or its main part, ought to be looked for, not in the introduction of the germ, but in the predisposition of the soil, that is, the state of the blood and tissues of the child, to stimulate its growth. Of course, I do not suggest that the germ was created by the suitability of the soil to favour its growth. The germ was not created by the soil, but it was, I believe, attracted by it. Or the germ may have been lying dormant in the soil (that is, the body), and, finding a suitable pabulum in its wrongly fed state, or over-manured state, took to growing in it. Now this is no imaginary case. I saw not long ago a little girl, five years of age, pale, thin, delicate, refined, and her mother told me that the child could not eat. "She only picks," she said, "like a dickey bird." And quite recently the child had had typhoid fever—at least, so it was said—and a doctor had attended her for it. Whether the child had typhoid fever or not, it was well that that opinion was formed and acted upon, because, as is well known, the best treatment for that condition is

an exclusive milk diet for several weeks, three, say, to six, and sometimes nine, and very occasionally even twelve. If only the milk is kept down to small amounts, say to a pint or so a day, diluted with hot water so as to make it easier of assimilation, and to let it be administered at the comfortable and convenient temperature of about 70° F., with perhaps half a pint of mutton tea or chicken tea in addition in the 24 hours—if anything is wanted in the night, water is best, for the stomach and digestive organs require rest as well as the other parts of the body—nothing but good can result. The child in question had been treated in this way, and had in due course recovered, her appetite returning in a more healthy way than before the illness. By and bye, however, the delicate and refined little thing began to want less food, and the mother became alarmed. In order to make up for a poor breakfast, she served the child with lunch at 10-30 a.m. of bread and milk, a wholesome food enough, but given at the wrong time, especially as the child was expected to eat dinner at one. Naturally enough the dinner meal was refused, or a mere pretence was made to eat it. The mother being again alarmed lest the child should die of starvation, had some more food served at four o'clock, and again at six. The child was having not too little done for her but too much. Injudiciously, but well meaningly, no doubt, she was having too many meals forced on to her, and the consequence was that her digestion became

entirely upset and deranged, her tongue became furred and dirty, and her appetite almost wholly disappeared. I recommended that she should be put to bed and fed as if she had typhoid fever. I did not say she had typhoid fever, because I did not think she had ; but I put her to bed and recommended poultices or compresses to her stomach regularly for an hour, twice a day, with a cool evaporating lotion to the head at the same time, and suggested giving not more than a pint of milk well diluted with water, and nothing else, in the 24 hours, and going on with this until the child began to call out for something to eat. This she did in about a fortnight, when, her tongue being clean enough to make it seem right, she had some dinner given her. The milk was continued as before, morning and evening, and the dinner was continued for several more days. After this, the child had a little bread with her milk about six o'clock, and soon became quite well, with a reasonable appetite for her dinner between twelve and one, and for her "tea" between five and six p.m. As she was a delicate child, with a low resistance to digestive labour, I recommended continuing this, really a two-meal-a-day plan, for some time; and said I thought it would be well if this course were adhered to for the future. Sometimes mothers are convinced by this sort of reasoning, and sometimes not. For some reason (perhaps because general medical opinion, and particularly the prejudices of friends, are violently opposed to it) it is sometimes

very difficult to persuade them to continue in this way, even although one can point to the facts that the child is doing well, plays with energy, and has a better colour, and more regular intestinal action, and that she wakes also with a sweeter breath in the morning, and also that she is gaining weight. Notwithstanding these clear evidences that things are going on quite well, in some cases mothers will not be convinced that they ought to let well alone. They begin again with the 10-30 meal "to keep the child's strength up, and to allow for growth," and they begin the day with breakfast at eight or 8-30 besides. In not a few cases, after one had been able to persuade the mother into rational ways of feeding children, grandmother has appeared on the scene and has sternly forbidden the continuance of the "pining process." Occasionally even the father has assumed this responsibility. Opinion is divided, as it was in Middlemarch, we are told, between Dr. Wrench and "the strengthening treatment," on the one hand, and Toller and "the lowering system," on the other; or we are reminded of the competition in Milby between Mr. Pilgrim and Mr. Pratt, "the latter of whom elegantly referred all diseases to debility, and, with a proper contempt for symptomatic treatment, went to the root of the matter with port wine and bark; while Pilgrim was persuaded that the evil principle in the human system was plethora, and he made war against it with cupping, blistering and cathartics." The present writer humbly agrees with Mr. Pilgrim

in his view, though not in his practice; failing to see how cathartics can clear plethora out of the blood and tissues, although, as they have some influence on the stomach and bowels, they might conceivably clear obstructions out of these, so ridding the system of the *materies morbi*, if it had not been for the other fact that, long before, the peccant matter had been passed from these organs and had found its way into the blood. On the other hand, Mr. Pilgrim's other sheet-anchors of blistering and cupping, while they might conceivably remove effete plethoric material from a small portion of the blood, or even remove some of that vital fluid itself, and along with it its deleterious accompaniments, could not, it seems to the present writer, exert any effect at all on the rest of the circulating medium, which would still continue to flow, loaded with its ill-made and unused stuff, to poison the tissues of the body. By restricting the diet, however, to a point under (for a time) what is required for the maintenance of the body in health, all these desirable ends may be attained, the non-introduction of new material compelling the organism to use up what is already in it, to the great benefit of all the actors and elements concerned in the problem. But in Milby, as in the rest of the civilised world, it is apt to be "the strengthening treatment" which commands the favour of a not too discerning or too closely reasoning popular opinion. "Port wine and bark," indeed, acting as stimulants, while they do not

supply the materials for oxidisation and combustion required by the body, might have an influence which neither Mr. Pilgrim nor Mr. Pratt, nor any of their respective supporters or partisans had ever considered, and might even be acting favourably in ways unsuspected either by the supporters or by the leaders themselves. A diet, in fact, of port wine and bark, or of alcohol in any form, or such a diet, in addition, say, to a pint of milk, and of soup daily, such as is not infrequently prescribed for patients suffering from various forms of fever and debility, is really a restricted diet, although it is called a stimulating one. Measured in food calories, it will amount to perhaps 300 or 400 food calories, as against the 2300 or so emitted by the starving man, or by the physician or the professor, or the literary man or woman, whose food requirements are so very low, as we have seen. And yet, such slaves are we to words, that a diet amounting in food value to only a fractional part of even the ounce of food-weight required daily for every ten pounds of body-weight, on the lowest computation, for the nourishment and enrichment of the blood, is thought of by us, if only we call it "a stimulating diet," as one in excess, and not as it really is, in deficiency.

But to return to the feeding of children; it is in many cases quite in vain to reason about the amount by which a child grows in a year, and to say how much extra food is required for this purpose; and equally useless to point to the various

marks of improvement, and to shew that he is gaining weight. The quantity of food which ought to be given to a growing child is too apt to be settled by prejudice rather than by reason ; and it is not often that parents can be induced to listen to the arguments which convinced the mother of the tuberculous child of four years, formerly referred to, who was cured of her necrosis of the thigh bone, and white swelling of the knee joint, on one meal a day and a glass of milk night and morning, and who gained nearly ten pounds in weight on this diet continued for fifteen months. That the two-meals-a-day plan, recommended by Dr. Dewey, is suitable for town-bred children in England, there can be little doubt. He proposes that children should go to school at say eight a.m., and stay there till ten, returning home then for breakfast. He points out that, their organisms not being troubled with the labour of digestion, their intellects and perceptions would be keen to take in and to remember the instruction they received. At ten he thinks they should go home to breakfast. At eleven they might return to school. I would suggest that they should be encouraged to play under cover if the weather is bad, and that in good weather they should be taken out into the country, if necessary a short distance by rail in the case of large towns, to study nature under the general superintendence of teachers, who should go with them and shew them animals, birds, plants, and rocks, in a conversational way, interfering no further with their liberty of free

movement than might be necessary to preserve order and prevent destruction or damage to property. At one or two p.m. they should be back in school, where for two or three hours more they might, by the help of books and maps, follow up the studies which they had practically, and almost without knowing it, begun in the fields and in the country. The scientific and orderly arrangement of knowledge, begun practically through eyes, ears, and fingers, might be elaborated and carried on by the help of books, &c. The stomach and digestive viscera, having done their work, the brain would not be distracted by having to superintend digestive labour, and would again, therefore, be almost as keen and receptive of instruction as it was from eight to ten a.m. In these two stretches of two hours each, young children would probably learn as much as they are capable of assimilating. Older children might have two stretches of three hours each, or perhaps one of three hours and one of two. At four they should go home to dinner of varied and mixed diet, and at six, seven, eight, or nine, they might go to bed, according to age. These seem very suitable arrangements to make children happy and healthy, as well as to educate them. The numbers of children of school age, about a sixth of the total population, say 50,000 in a town with 300,000 inhabitants, are numerous enough to demand arrangements suitable for their life, and health, and happiness, and instruction; and there would, it seems to me, be little difficulty in

adapting the business arrangements of the parents so as to make them suit all parties. But if this is thought too revolutionary a measure, there is nothing to hinder the present arrangements about breakfast and dinner, at eight and 12-30 or one, respectively, to be continued, and that the children should have either a glass of milk at 5-30 or six, or that and some bread, so that they should have in no case more than three meals daily. The occurrence of illness might reasonably be made the test as to whether any further change should be made. As disease is more often induced by wrong feeding, and by too frequent feeding, than by any other cause, and as this is as true for children as it is for grown persons, the occurrence of illness should in most cases be met by restriction of the diet, and this could best be effected by omitting either the morning or the evening meal, and substituting for it say a glass of milk. In these cases where a three-meal-a-day plan (it would be shorter to say a trissiteous plan) suited, we could follow the good conservative rule to let well alone; but in no case do children require to be fed oftener than three times a day. I do not, however, even in the case of children, conceal my own preference for the two-meal-a-day plan (or the dissiteous plan), thinking that they would be healthier on two meals a day than on three. If, however, they took three meals daily, this plan might be continued till the age of 21, or possibly till 25, after which, I think the two-meal *regime* should be resorted to, say till the age

of 50, or perhaps 55 years. After this, I believe, in the majority of cases, one moderate meal of mixed diet, taken some time between twelve noon and two p.m,, would be the healthiest plan. This could be continued to advanced age, say 75–80. After that age, many aged persons I have known have seemed to think that they obtain the best health results if they take a very little food three or four times a day; and one ought to be careful of recommending a line of treatment contrary to the views of those whose experience entitles them to know. True, the persons whom we meet who have attained advanced age, have seldom done so on a two-meal-a-day plan, or on a one-meal-a-day plan. It might be otherwise if they had. They have generally, however, done so on a somewhat abstemious diet, as Cornaro's case so markedly exemplified. There are, no doubt, exceptions to every rule, and one finds a few aged persons who have eaten freely all their lives, just as one occasionally finds also persons taking alcoholic drink freely and still surviving to old age. But these cases do not form the majority, and, when they are found, one is disposed to ask the question, how much better might they not have been if they had kept better within the bounds of moderation. That old people bear abstinence well is a very ancient observation, followed in the next sentence by the statement that they bear over-feeding badly, or, as the ancient writer put it, seeing the paradox which has occupied us so long in a former part of this

essay, they waste or consume away by much (*ὑπὸ μὲν γὰρ πολλῶν ἀποσβέννυνται*). And as there is no strict line of demarcation between infancy and childhood, nor between childhood and adolescence, and maturity, and elderliness, and old age, each age insensibly leading on to the one beyond it, temperance and restraint in the use of food seem to be the best means to be followed by humanity at all its stages of progress from the cradle to the grave, if life is to be healthy, happy, and useful.

CHAPTER XI.

On Cancer.

I HAVE thought it well to introduce a chapter on cancer, because it is occupying a large amount of public attention at the present time. Perhaps I ought not to say that it occupies a large amount of public attention. I ought rather, I think, to say that it is causing a large amount of public panic. I wish cancer did occupy a larger share of public attention, because I have come to think, and the reader who has cared to accompany me so far will be prepared for the statement, that cancer is not a diseased condition of body to be considered by itself; but, on the other hand, being only one of a large number of diseases that overtake humanity, and obeying the same laws as other diseases, it is induced by similar causes, and, like them, though not often cured, to a very large extent it is obviable or preventible, and this by the simplest means. I share the general opinion of the greatest of medical writers, as the reader will have observed, that disease is, on the whole, one; and I venture to translate this opinion practically into the view that on the whole the causes of disease, of all diseases, are the same, and,

therefore, that the means, if not of curing, at any rate of preventing cancer, are the same as the means of preventing other diseases. Therefore, also, I believe that proper attention to air, food and exercises would do as much for us in preventing cancer as it would or could do for us in the case of other diseases. It seems to me a very simple matter; theoretically, I mean, for practically it involves the problem of self government, it involves a certain amount of self restraint; and no one who thinks about that, or studies his own nature, can fail to perceive that that (while it is the main problem in life) is at once the simplest and yet the most difficult of all problems. Panic is, however, the least favourable attitude of mind in which to approach the consideration of any subject. I deprecate panic in the consideration of this one. In the number of deaths which it causes, cancer is not even one of the most important of the diseases to which humanity succumbs. If we consider the effects of the fevers, of consumption, and of cancer, on the people of England, we may roughly say that fevers cause four deaths for every one caused by cancer, and that consumption causes two for every one of cancer. That is to say, cancer causes only half of the mortality caused by consumption, and only one quarter of the mortality caused by the fevers. But we have on the whole greatly diminished the incidence and the mortality both of the fevers and of consumption; and there is, therefore, on the face of it, no reason to fear that

we shall be able to do the same for cancer. I have not the slightest doubt of this myself. Neither do I think that it is necessary for us to raise either a million pounds sterling or a hundred thousand pounds in order to do this. Any results to be obtained by expensive laboratory work, by microscopic and bacteriological inquiry, seem to me more or less useless, practically, or from the point of view of the causation and prevention of cancer, which are the only really important aspects of the question. Such inquiries are academically interesting, no doubt; and all kinds of knowledge, I freely admit, mutually illuminate one another. But when laboratory results are obtained, divorced, as they too often are, from every day life, all the use that can be made of them will be to corroborate the conclusions which we much more simply and much less expensively draw from the occurrences, from the facts, and from the observations we may make from every day life. They can, at any rate, never, without subverting the true order of nature, be made to supersede the latter. A striking illustration, I think, of the truth of this view may be obtained from the history of another disease. Diphtheria is now believed to be caused by the growth in the body of a certain micro-organism, the Klebs-Löffler bacillus. (The view I would put before the reader is that diphtheria is caused not by the bacillus, but by the state of the body which makes the bacillus flourish.) Our health authorities, therefore, following out logically the view they

hold as to the causation of diphtheria, provide practitioners with tubes, &c., in which to collect material from the throats of patients suffering from this disease, and kindly examine them for us at a public laboratory, free of charge. They seem by their attitude almost to wish to compel us to treat the bacillus by injections of serum, and in a manner to force the hands of experienced practitioners to this mode of treatment, as if they thought that it is the disease which is to be treated, or the bacillus and not the suffering patient—but I shall let this consideration pass. Now what happens? This, among other things. The doctor gets a case, takes a swab from the throat, sends it to the laboratory, and, in due course, gets a report that it swarms with bacteria of the most deadly sort. All the while, however, there are no symptoms in his patient which cause him the slightest anxiety. Then he has another case, or his colleague has, at the same time; and on this case the report is that the diphtheria bacillus is only doubtfully present—yet the patient dies in spite of serum injections and the most approved modes of treatment. (Very possibly the patient's blood and tissues were in too bad and depraved a condition to allow the bacillus to grow? I suggest, in fact, that we have not yet learned how much importance or how little we ought to attach to our recent bacteriological discoveries). These are not imaginary cases; they occur every day when diphtheria is abundant. What inferences are we to draw from them? Why this inference,

among others, that the presence or otherwise of the diphtheria bacillus is comparatively unimportant; and that on the other hand the totality of the symptoms and the general condition of the patient are of the utmost consequence. That is to say, that the laboratory inquiries are not nearly so important as the clinical history and conditions of patients, and that they must be corrected by the latter. The same conclusions, I have not the least doubt, will be arrived at regarding cancer; and after the expenditure, not of a hundred thousand pounds, but of a million pounds, we shall not discover anything useful in the prevention and treatment of the disease in this way that we might not have discovered far more simply from clinical and practical observation. For note; the laboratory investigation throws no light on the conditions *in the body* which make the diphtheria bacilli grow or fail to grow. Nor will any amount of microscopic, and laboratory, and bacteriological investigation into cancer be likely ever to tell us what causes make it occur in the body—while, on the other hand, a very little practical and bed-side investigation gives us a speedy answer to the question. At least I think it does. And it is at least reasonable to ask inquirers to attempt to answer the question in this way before they make demands for funds, the proper application of which they have not sufficiently considered. Some light on the question whether cancer is accompanied by the growth in the body of a micro-organism or micro-

organisms might be shed by this method of investigation. And if the micro-organism proved to be always the same one in cancer, more light might, no doubt, be obtained as to the diagnosis of the disease. But of what value would improved diagnosis be? Of a scientific or classification value, no doubt it would be; but of practical value, none, or next to none. What we want to know is, what causes acting on the organism favour the production of the diseased state, be it connected with the growth of micro-organisms or not? And what conditions, on the other hand, are unfavourable to it? In short, what causes it, and what, therefore, will prevent it? Now these questions are physiological and clinical; and we already, I think, possess all the knowledge that we require to answer these important questions. And to give an answer at once and shortly; it may safely be said that health and soundness of the body render it an unsuitable site for the growth of micro-organisms, while unhealthiness and unsoundness, on the other hand, render it suitable for such growth. And healthiness and soundness of body are to be obtained by maintaining proper relations between the body and its environment, and particularly by suitable relations between it and air, food, and exercises. And if, on the other hand, unsuitable relations between the body and air, food, and exercises, have subsisted for say 20, or 25, or 30 years, not all the knowledge conceivable, or to be attained after the expenditure of any amount of

time and labour, and of any conceivable amount of money, into the natural history of micro-organisms, will enable us to undo the evil effects of wrong living carried on for so long a period of time. Let us at least look at the facts presented to our clinical observation, of the habits of the people who suffer from cancer, before we ask for money to enable us to institute expensive laboratory inquiries—let us use our eyes before we inquire with the microscope—and if, after that, we wish to draw on the millionaires, or if they are willing to be drawn upon, by all means let them be invited to hand over their money.

A use to which a sum of a million pounds might be put in order to investigate the causes of cancer might be the following. I fear the donors of it would not consent to a use so simple, but I have more fear for the demanders than for the donors. It would not be very easy to carry it out perhaps. What I would suggest is that a large number of townspeople at all ages, say 10,000, should be fed five times a day, say at eight and eleven a.m., and two, five, and eight p.m., on bread and jam for a generation. Let at the same time a similar number of people, similarly circumstanced, be fed twice a day only, on ordinary mixed food, to the amount of not more than 16 ounces avoirdupois a day, and let the diseases and illnesses of these two groups of people be compared and tabulated every five years for a generation. I know in which group the largest number of cases of cancer would occur. And I

also know in which group the largest number of cases of other sorts of disease (yes, including consumption and the fevers), would occur. At the end of the time we should all know it also; that is, if we wished to know it. But as this experiment is really being made all the time, is it not better to use our observing faculties to see what is going on, without troubling our millionaires to part with their money, or our caretakers to take all the trouble that would be involved in making such an experiment, necessary as it would be to carry it on over a very long time?

No doubt cancer differs both from the fevers and from consumption in this respect that it claims its victims mainly among the mature, among the fathers and mothers of families, among those whose presence has come to be most important to their families, and so to the community; while fever and consumption deal with those whose lives are yet for the most part in promise, rather than fulfilment. This increases the importance of cancer in a way which the number of its victims does not justify. It was no doubt because he felt this consideration weighing upon him that the King, on the 25th July, 1901, in welcoming to London the Congress which met there for the purpose of studying tuberculosis, and mitigating its effects, after some sympathetic words regarding the objects of that Congress, went on to say:—

"There is still one other terrible disease which has, up to now, baffled the scientific and medical

world, and that is cancer. God grant that, before long, you may be able to find a cure for it, or check its course. I think that to him who makes the discovery, a statue should be erected in all the capitals of the world."

The candidate for that statue is still to be discovered, if, that is, the finding of a cure for cancer is to be the only cause for which the statue is to be erected. If, however, checking of the course of cancer is to be considered worthy of the erection of a statue in all the capitals of the world, that is quite a different matter. To check the course of cancer would not be difficult if the cause or causes were known, for by obviating its causes, the disease could be prevented; and prevention would be the most effectual means of checking its course. That at least the predisposing causes of cancer are known is a matter of conviction with the writer of this essay, and he thinks that this conclusion will be shared by candid readers, after consideration of the evidence, the probable evidence, which has weighed with him.

After what has been said already regarding predisposing and exciting causes, and the differences between them, I need not weary the reader by recapitulation. As it is the predisposing causes which are important, it is to them that our attention must be directed. The suggestion is that by acting in certain directions, which are easily within our own power and control, we might be able to prevent the incidence of a large proportion of

the cancer which now afflicts us. Cure of cancer, after it has once occurred in the body, is a very different affair from prevention. Prevention may be easy ; cure impossible, or practically impossible. These are, in fact, the conclusions which I wish to set before the reader.

A preliminary word may, however, be permissible regarding prevention, in general, of cancer, and of other diseases. However well the causes of disease may be known, and however well we may grapple with them, or strive to prevent their action, it is scarcely to be hoped that we shall ever absolutely prevent the occurrence of their effects. The complete prevention of cancer is not to be looked for. There will always be anomalies for which we shall be unable to account, always some cases which it will be impossible to avoid. Some unknown and probably undiscoverable causes in the history of various individuals, or in their ancestry, some instances of the unknown "personal equation" will arise, which will make it impossible for us to prevent occasional cases of this disease. Cancer does not differ from other diseases in this respect. The fevers, for example, and consumption are held to be preventible diseases; but they have not been prevented, although their incidence and mortality have been considerably diminished ; and although I think that much more might be done in this direction than has yet taken place, if we were somewhat to enlarge our conception of the causation of these diseases beyond the view which

attributes them to bad air and over-crowding, still I do not expect that we shall ever entirely put an end to either of these plagues of humanity. Already consumption has fallen as a cause of mortality from 2800 per million per annum, at which point it stood at the time of the Crimean war, to 1325 per million; and no doubt it will fall still further, but it will be utopian to expect that it will ever entirely cease from among us. Some cases, I suppose, always will occur, as they always have occurred in the past, in the long history of the relations between the organism and its enviroment, for it is improbable that even if we knew quite all the causes of our illnesses, we should ever be able to entirely obviate their action or neutralise their effects. The same considerations must govern us in the case of cancer. A diminution by one half would be a great achievement. A reduction by 75 or 80 per cent. might, almost without straining of language, be called practical prevention. In the decennium, 1861-70, there died of cancer in England and Wales a proportion of people amounting to 384 per million per annum persons living. In 1871-80 the proportion had risen to 468 per million per annum. In 1881-90 the number was 589, and the ratio is still rising. In the five years, 1891-5, the ratio, for instance, was 712, and in the four years, 1896-9, the latest date for which figures are available, it has risen to 795, an increase of more than double, if we compare it with 1861-70. It is not necessary here to consider the question whether

the whole of this great and alarming rise is real, or whether it is only partially so. Allowing for whatever part of the increase can be accounted for by better diagnostic power and better naming of diseases, it still does not seem possible to escape the conclusion that this disease has increased steadily as a cause of death for the last forty years. Now while the writer does not look for anything like complete prevention, he does not see any reason why it should not be reduced again to the rate at which it stood say in 1861-5 (368 per million per annum), or even why it might not be reduced still further than the rate then obtaining.

The first question, then, which we have to discuss, is this: Is the discovery of a cure for cancer an object likely to be achieved, a goal possible of attainment? To the question put in this form the writer's answer is negative. The search for the cure or for a cure for cancer is not a hopeful role to follow. It is in fact an object impossible of attainment, or at least so unlikely to be attained as to render the search for its attainment futile. We may as well spare our pains. The writer's view is that when cancer has set in, when the body physical has become cancerous or affected with cancer, the organism has become so profoundly modified in an unhealthy direction that to hope or work for cure is to hope or work for that whose attainment is impossible. Perhaps he ought not to use the word *impossible.* It is not necessary to contend that no case of cancer has ever been

cured. That would be to go beyond the evidence. Occasionally, though very rarely indeed, probably persons affected with cancer have been cured, or at least have recovered. But the view of the writer is that cancer has been so rarely cured that it is far nearer the truth to say of the organism—once cancerous always cancerous—than to say that it is likely to be helpful or profitable for us to embark on a voyage of discovery to obtain a cure for this disease. Instead of *impossible* the writer would prefer the expression *practically unattainable. Impossible* raises metaphysical or super-sensible or ultra-scientific considerations (which considerations, however, we cannot help raising from time to time, however difficult their solution may be), and these would be shewn to be inadmissible if but one case of cancer had been proved to be cured—a possibility which, as he says, the writer is not concerned to deny. It is not what is abstractly possible that will profitably occupy our attention and our efforts, but what is likely to be practically attainable in the light of our knowledge of causes, and of that which has hitherto been attained. The writer's view is that "once cancerous always cancerous" is a position of mind and fact far nearer the truth than that the discovery of a cure for cancer is likely to be made. Certainly if operation is to be persisted in for the eradication or obviation of the effects of this malady, some addition will have to be made to this method of dealing with it, some advice concurrently given which shall enable the

patient to get outside of the action of those causes which have induced the disease in the first instance, and whose obviation may enable him to prevent the recurrence, which, though sometimes delayed for many years, and very rarely, perhaps, obviated altogether, takes place in the large majority of cases in from six months to four years. If operation alone is more or less futile in curing cancer, perhaps operation coupled with the advice to live differently in future might have some chance of effecting a cure? When His Most Gracious Majesty made to the scientific men assembled in his capital the suggestion referred to, he no doubt spoke out of a full heart, which was still lamenting the death of a younger brother from that disease, and at the same time was looking hopelessly forward to the certainty, which has been unfortunately too sadly realised, that an elder sister was about to succumb to the same malady. Without discussing what is theoretically possible, the experience of the royal families of England and Germany emphasises the futility of the treatment of cancer in the present state of knowledge, for if these powerful families could not command the means of cure, it may well be asked—who could? In the writer's view, the causes of failure to find a cure were and are inherent in the nature of the case, and the fact that the best skill available, medical and surgical, failed to save the patients, was no accident of the situation, but an ultimate factor to be dealt with in at least the great

majority of cases where changes have advanced so far in the organism as to result in the formation of the cancerous state. It will be seen, therefore, that, for reasons which he believes will strengthen as his argument proceeds, the writer thinks that the search for a cure for cancer is not a hopeful object of research; and, in fact, he believes it to be practically impossible of attainment. He is no candidate for the statue to be erected in all the capitals of the world. If through a flaw in the bank of a great reservoir the confined waters had burst their bonds, had devastated the neighbouring country, had destroyed growing crops, had drowned cattle, swept away houses, and carried to long distances the corpses of their occupants, it would be obviously futile to suggest the erecting of a statue to whosoever should cure the effects of the devastation, since indeed the effects would be incurable. Nothing ever would or could restore the crops, the houses, the cattle, or the men. But it would be hopeful—and if the erection of a statue in all the capitals were considered a fit reward for the service, the candidate for the statue might easily be forthcoming—it would be hopeful to inquire how the devastation occurred, where the flaws in the banks were, and by repairing and strengthening the weak place or places, to prevent a recurrence of the evil whose effects unfortunately it would be too late to remove or undo. Especially would this be so if not one but several devastations had successively occurred, for no donbt proper

arrangements would prevent such recurrences in the future. The parallel between the effects of such a devastation, or such a succession of devastations, and the ravages of cancer, appears to the writer to be very complete. To cure it appears to him almost as hopeless as to attempt to undo the effects of the devastation. The proper aim and role of medicine is not to attempt to undo effects without reference to causes, although unfortunately we too often act as if we thought it was. Her true role really is, by discovering the causes of the maladies from which we suffer, to prevent the effects by removing the organism outside of the sphere of action of the causes. In fact, the enlargement of the sphere of preventive medicine is a much more hopeful object of attainment to true medical science than the discovery of cures. Cures are effected by nature, and the chief function of the medical adviser is to interfere as little as possible with the beneficent means of cure adopted by nature.

By cancer, the writer means chiefly carcinoma or epithelioma, an overgrowth of epithelial or superficial structures, two or more epithelial cells appearing instead of one. This condition, when once it has commenced, goes on to more and more, constantly spreading, ulcerating, or fungating by advance and recession, the advance, however, unfortunately preponderating, till in time the disease destroys the life of the person suffering from it. As to sarcoma, as distinguished from carcinoma, the most illuminating idea, in my

opinion, is the view that sarcoma is merely cancer of the connective tissue. This idea simplifies the problem very much, since on this view cancer of epithelium is a special form of hypertrophy of the epithelial tissues, and sarcoma is cancer of connective tissue. So that cancer and sarcoma are probably both due to the action of the same causes on different tissues; and the investigation or study of the one is the same, *mutatis mutandis*, as the investigation or study of the other. But to call carcinoma or sarcoma a hypertrophy or overgrowth is to endorse the writer's view that they are an overgrowth due to over-nutrition. And no doubt cancer and sarcoma *are* hypertrophies or overgrowths. In the one case there is a large increase in the amount and number of the epithelial cells, the appearance of two or more epithelial cells where only only one ought normally to be; in the other case there is a large increase in the number of connective tissue corpuscles. Perhaps, on the same lines, colloid cancer might be considered a hypertrophy of the mucoid cells, and encephaloid cancer of some other forms of cells? Parasitic cells might or might not appear among these cells, according as it happened. But the essence of the disease on this view is the hypertrophy; the occurrence of parasites the accident. Now the question is, what is the chief predisposing cause of overgrowth? Well, what *can* be the cause except an excess of materials in the blood? And if so, whence came the excess of material which is poured

out of the blood in the form of the cancerous exudation? What source can there be but the environment of the organism? And of all the facts of environment, what so likely to be the chief cause of change in the body as the food?

A recent writer on cancer, in advocating the infection theory from without, says that "no alleged cause can possibly be accepted which is not universally applicable, and infection from without is the only suggested cause which will satisfactorily account for every case of cancer." Well, this is not incompatible with my view that over-feeding is the predisposing cause of cancer, because it is even more "universally applicable" than infection, and because the over-fed state of the organism is an efficient cause in making infectious diseases grow in the body as noxious weeds grow in the soil, from over-manuring of the ground; and certainly over-feeding is introduced "from without."

As regards sarcoma and the view that it is a connective-tissue-cancer, how illuminating and how important become the remarks formerly made, that this same connective tissue is the first great place or part in the body where the products of an excess of food materials, finding their way into the blood, are primarily deposited. We called the connective tissue the great dumping ground of the blood, the place which was, so to say, chosen by the blood as the least hurtful place or site in which to lay down any excess of material which it might be carrying, and for which it had no use. Under the

name of initis, we shewed how connective-tissue-congestion was the basis of such states as neurosis, hysteria, rheumatism, gout, &c. And we further shewed that the next stage after initic congestion was effusion, as *e.g.*, into joints, or into the origins, or insertions of tendons about joints, or into such cavities as the pleura or peritoneum, or the membranes or meninges of the brain. Now we see that when connective-tissue-hypertrophy or over-growth takes place in any particular part of the body, sarcoma may occur, and from these facts becomes more apparent the connection between diseases so very different (at first sight) from one another as sarcoma and the diseases just named. From which considerations a corroboration arises of the view that disease is essentially one, with many different phases or aspects, a view which has been held in the past by some of the greatest thinkers and writers on medicine Changes in connective-tissue-cells form the one common factor uniting these so various diseased states; and changes of this sort are brought about by changes in nutrition, that is, in the feeding of the body. It is food supply, therefore, which is the chief factor in inducing health or disease in the body, and to alterations in this (and usually to restrictions) must we look if we are to do anything towards relief of the fatigue that accompanies initis, or towards the prevention or diminution of the hypertrophy that characterises sarcoma and epithelioma.

It may be well to inquire here, since cancer

and sarcoma seem to be made out of an excess of material finding its way into the blood, what are the chief ways by which such a process is likely to occur, how, in fact, any excess of material or, indeed, how any material at all may find its way into the blood. There seem to be three or perhaps four channels through which such entrance may be effected. (*a*) Foreign matter may enter the economy through the respiration; it may be inhaled or breathed in. (*b*) It may be inoculated, or it may find its way in through some crack in the skin, or in a mucous surface. (*c*) It may be introduced through the ancestors. (*d*) It may enter through the mouth; it may be swallowed with food or drink; or, an excess of food, being ingested into the digestive tract, may cause some form of disease, and may require to be eliminated in some way. To take these different methods in order, shortly, let us inquire, is it likely that cancer is breathed or inhaled into the system? Is it likely that some germ, for instance, lighting upon our clothes, may be inhaled as impalpable dust, and do its deadly work after its reception into the economy? This is a possible mode of the introduction, no doubt. There are other affections whose introduction into the economy is believed to be, at least sometimes, effected in this way; whooping cough, for example, consumption, and others. But is it likely that carcinoma is introduced in this way? The writer thinks not. Affections so introduced, or rather, bodily con-

ditions so induced (for disease is not so much an entity superinduced on the body, or added to it, as a state of body somehow induced, a departure from health), generally run a rapid course, extending over a few days or weeks, and accompanied by notable feverishness. Neither of these facts is characteristic of cancer, whose onset is usually insidious, and its course slow and unaccompanied by feverishness. In fact, so far from the temperature being raised in cancer, it is usually sub-normal for years before the onset of carcinoma, only rising above normal as intercurrent inflammatory attacks occur. And the temperature appears to be sub-normal, because the presence of the effete materials in the blood and tissues, which induce the diseased condition, lower vitality and crush down the powers of the organism, like a horse overburdened under the load which it is carrying. Another objection, not of great weight, perhaps, in itself, but helping as a contributory consideration to induce a negative answer to the question whether cancer is introduced through the respiration is, that if it were so introduced, we should expect cancer to affect the respiratory tract more than it does. But although cancer does affect the lungs sometimes, it does so far too seldom to make it seem probable that the diseased condition is produced in this way. Even if it does, however, or if it did, all the considerations formerly noticed regarding the mutual relations or reciprocity of soil and seed would still remain and hold good;

so that as regards prevention of such a possible mode of infection, the measures proposed would bear relation chiefly to the germ on the one hand, or chiefly to the state of the person on the other, according to the importance one attached to either of these factors. On the one view, we should advise the person to avoid and keep clear of infection; on the other, to get his body into such a state that if a cancer germ or two did light on him, he would take no harm from it. It will, I hope, now be abundantly evident which of these two views I should take..

(*b*) Is it likely next that cancer is introduced into the economy, or that the economy becomes cancerous by inoculation through a crack in the skin, or by endosmosis or absorption through the skin or other bodily surface? If so, should we not be able to point to the spot at which the inoculation was effected, and to prove the onset of the disease in this manner, as also to detail its further progress? The evidence that cancer is introducible in this way will be dealt with immediately. At present all that need be said is that it is not a very likely mode of cancer origin, although it certainly cannot be pronounced to be an impossible one.

It may appear to some that lip cancer, or epithelioma of the lip, so frequently associated with the smoking habit, that it is rare indeed to find it in non-smokers (I have only once seen it in a woman, and she smoked) affords a case in point,

where the cancerous germ is or may be introduced into the blood, through a crack in the lip. But another explanation is possible, and quite as reasonable, if not more so, viz., that the smoking acts only as an irritant to the lip, the excess of activity thereby induced to the part causing an increased flow of blood thereto, the blood depositing therefore at that spot by preference some of the waste or unused material which it had in it in excess, so forming the epitheliomatous growth. There is no evidence that the tobacco introduced the particulate matter which caused the epithelioma, any more than that the basic slag formerly referred to introduced the clover seed, or that the railway cutting introduced the seeds of the new flora, afterwards found in the district. It is, however, quite reasonable to suggest, and, I may add, it is a sufficient explanation, that the irritation, by heightening vascular activity, acted in the way supposed. There are numerous other analogies to bear out this view. For instance, the occurrence of cancer is often attributed to a blow; but no one suggests that, for example, a blow from a child's arm, causing cancer in the mother's breast, actually introduced a cancer germ at the time. Such a thing is highly improbable, and indeed practically impossible. Then again, take this case which actually occurred in my experience. An old gentleman of 87 years of age, was knocked down by a bicycle, and the injury was followed by an attack of gout. The obvious

explanation of both of these cases (and it is quite a sufficient one), is that the blow followed by cancer, and the bicycle injury followed by gout, were in both cases the occasion indeed of the malady, but they were not the cause (*πρόφασις οὐκ αἰτία*, to use the words of Hippocrates), the cause being the state of the patient's blood in both cases, loaded as it no doubt was by the presence of waste unassimilated materials. Otherwise we may say the injury was no doubt part of the cause, but only a small part, the chief part being the state of the patient's blood and tissues.

I ought, however, to discuss another phase of this question before leaving it. Experimental inoculation of cancer appears to have been successfully performed in the lower animals. Watery emulsions of materials taken from cancerous growths, on being injected into the veins of dogs, have been followed in course of time by the appearance of cancerous growths in the lungs. Well, I can admit the success of these and similar experiments without any damage to my theory that over-feeding is the chief predisposing cause of cancer, because I do not deny that, after cancer has been produced in the body, it may spread by infection from a given site. But I wish to point out that cancer in fact never does come in the human subject from inoculation, except in one or two very rare and not quite certain cases, in which surgeons may have inoculated themselves. I must say that such a risk in operating troubles me very little. I should think

that the state of the operator's health had more to do with such a mode of origin than the inoculation which was alleged to cause it. And, besides, our problem is to account for the original origin of cancer and sarcoma, rather than to say how it may behave and how it may infect the body after it has begun to grow.

(*c*) Has carcinoma been transmitted from ancestors? Is it being so transmitted now? Is it a hereditary disease? If this were so, should we not find cancer running in families more than we do? And should we not also find cancer appearing oftener than we do in the childhood of the children of those who have themselves suffered from the disease? But there is no evidence of this. Even if it *were* so (and I have already discussed this question in dealing with the general subject of the heredity of disease) and if the children of persons suffering from cancer were themselves cancerous, it would not follow that the disease was transmitted, because it might have been, and probably, in fact, would have been the similarity of environment in parent and children, the similarity of the conditions to which both were subjected, which induced the affection in all the cases. Where, however, is the proof of the hereditary transmission of carcinoma, when in so many cases grown up descendants die of quite different affections? of pneumonia, for example, of infectious fevers, or in advanced life? I need not repeat what I said before in dealing with the general question. I will

just remind the reader that the occurrence of cancer in childhood is very rare indeed, while we should expect it to be common in childhood if it were hereditarily transmitted, since young children are nearer to their parents than they are later, and may therefore be expected to be more affected than afterwards by inherited conditions. But if cancer appears after 50 years of age in the descendants of those persons who may have suffered from cancer, we shall have to answer to ourselves this question: whether is it more likely that a disease, appearing after half a century of life, or more, has been transmitted from ancestors; or, is it more likely that it has been acquired, that is, produced by some condition in the environment of the persons who suffered from it? By some habits of their own, in fact? Put in this way, there can be but one answer to the question, viz.: it is far more likely that it has been acquired. In the case again of parents suffering from cancer, and whose children may also suffer from the disease, other considerations arise. In many instances the parents did not acquire the disease till long after the birth of their children. Children may be born when their parents are 35 or 40 years of age, and when the parents were quite healthy, so far as could be known. But at 55 or 60 years of age, when the children were 20 years old, or so, the parents, let us suppose, develope cancer. Is it likely that in such circumstances parents will transmit to their offspring an affection which they did not themselves suffer

from for 20 years after their children were born? There is surely no evidence for this, on the face of it, unlikely view. It will be said, they transmitted, not the disease, but the predisposition to it. But what is the meaning of predisposition? Predisposition means inverse resistance; it means that the children in such circumstances took on cancer more readily than other and ordinary children would have done. But the proof of this is very hazy and very vague. The theory is started to account for a set of facts, of which a far more likely explanation is at hand, viz.: that similar conditions of environment induced the diseased conditions in the parents, and many years afterwards in the children. The ancestors were not to blame. They transmitted to their children the same sound constitution which they themselves possessed at the time of the birth of their children. They did not then suffer from cancer. How could they then transmit that which they did not themselves have or possess? Obviously this phase of the question only requires to be stated in order to be answered in the negative.

But further. If parents suffer from cancer at the time of the birth of their children, and if the children suffer in turn from cancer, but not till after they are 50 years of age, is there any proof of the transmission of cancer in this case? Very little. Organisation is transmitted, indeed, and any human being may take on any human condition or disease, as every human being is predisposed to every

human ailment; but as to the inheritance of disease, what is the proof or evidence for that? Sometimes, indeed, though very rarely, disease does seem to be transmitted, as when a family-disease appears in the children at birth, or immediately after it, and before the causes which generally induce it have had time to act and to bring it on. But how rarely does this occur. No doubt when it does happen, it is a very striking fact, and one which makes a deep impression on our minds. As, however, I have already dealt with the general question of the hereditary transmission of disease in the chapter on heredity, I will not go further into it now, contenting myself with the statement of my two or three contentions, (1) that although disease seems sometimes to be transmitted from ancestors to offspring, this very rarely happens; (2) that as a rule organisation is transmitted and disease not; and (3) that the chief law of heredity in disease (and in cancer, therefore, as a form of disease), is this: that like causes acting on like organisms in succeeding generations induce like effects.

To see now as an actual fact what probability there is of cancer being transmitted or inherited, let us consider the following statements. Of the 20,353 persons who died of cancer in England and Wales in 1892, only 103 were under five years of age. Of the 21,135 who died of this disease in 1893, only 94 were under five years of age. Of the 21,442 persons who died of it in 1894, only 103 were under five years of age; while of the 22,945

who died in 1895, and of the 23,521 who succumbed in 1896, only 115 and 110 respectively were under the age of five years. That is to say, out of the 109,396 persons who died in England and Wales of cancer in the quinquennium of 1892-6, only 525 or ·48 per cent., not quite one in two hundred, were under five years of age. Surely if cancer were a hereditary affection, a larger proportion than this of cases of the disease would have occurred in early life, since children, being nearer to their parents than they become later in life, would be more likely to manifest congenital characteristics. If, however, the causes inducing cancer take some time to act, and if the affection is acquired and not hereditary, we can see how it should become, as in fact it does, increasingly fatal as age advances, that is, in proportion as the causes which induce it have longer and longer time to act. If, then, it does not seem very likely that cancer is inhaled into the economy by the entrance of some germ through the respiratory tract; if there does not seem much likelihood that it enters the body by being absorbed through the skin; or if the fact that so few cases occur in infancy discredits the theory of its hereditary transmission, are there any other suppositions open to us? Seemingly, the causes of cancer, whatever they are, take time to act. As has been said, deaths from cancer are increasingly numerous as age advances. In 1900, for instance, which shewed the same characteristics as are seen in other years, there were only 502 deaths from

cancer under 25 years of age out of the 26,721 deaths from cancer in that year. From 25-35 years of age, 737 deaths occurred. Between 35 and 45 years of age, 2551 deaths occurred from cancer; between 45 and 55, 5501 deaths occurred from cancer; between 55 and 65, 7745 deaths occurred; and between 65 and 75, 6631 deaths occurred. That is to say, the causes of cancer, whatever they are, increase as life advances, reaching their culmination in the middle period of life after 45 years of age, and between that time and 75.

(*d*) The fact that (practically) cancer is not a children's disease has insensibly brought the inquiry up to the fourth way in which foreign material may find its way into the blood, viz., through the digestion. Does cancer enter the system through the digestion? The facts regarding the age-incidence of cancer appear to me to be susceptible of a very simple explanation, as do most, if not all of the other facts regarding the incidence of the disease. Children appear to me not to suffer from cancer (practically, of course, for we have seen that a few cases occur) because they have not had time by their food habits to induce it; while it increases as age advances to the middle period, because the wrong feeding which, I suggest, is at the bottom of it, and is the chief cause of it, is a cause which goes on gaining power as age advances. In childhood and early life, irritation of the organism is accompanied by intolerance, and therefore we find so violent a reaction from the

over-feeding of children that the infectious fevers and inflammations, sudden and fierce, and very often of short duration, are the characteristics of childhood. Up to middle age this cause goes on increasing. But as the bodily tissues harden and become more resistant with advancing time, they bear irritation better; irritation is accompanied by tolerance, and so we find the diseases affecting this period of life less fierce than in early life. The bodily temperature, instead of reacting, as in childhood it mostly does, into elevation above natural, is frequently characterised by depression below natural; and so one of the chief characteristics of the pre-cancerous stage is the existence of a subnormal temperature. Even in childhood this condition is less uncommon than is believed. It very often precedes the onset of tubercle, say of a joint like the knee, or of some other part. But in middle life this condition is much more common, as it is even in adolescence. The organism being weighed down and oppressed by the excessive load it is being compelled to carry, and the tissues being somewhat resistant, it does not intolerantly react against the irritation into high fever, but on the other hand is simply depressed by it. This is the time to treat the condition by restriction of the diet, in order to reduce the irritation, although, unfortunately, the opposite advice is too commonly given, viz., that the patient should be fed up in order to elevate his temperature. Then the reasons why cancer becomes less common

over 65 years of age are simply these. Persons by that time have learned how to live. Those who have not learned, or who would not learn, have been swept away by some of the chronic, or by some of the acute illnesses; and nextly, of course, by the time 65 years has been reached, only a small proportion of those who started life together now survive, so that the numbers of those capable of suffering from cancer must be much reduced. On the other hand, of those who do survive at 65 years of age, a large proportion suffer from cancer, owing mainly, I think, to the wrong methods of living so much in vogue.

It is said that among domestic animals, dogs suffer from cancer to a considerable extent, while pigs scarcely suffer at all. If this is the fact (I do not myself know, but I am told it is so) a likely suggestion, germane to what has been just said, occurs to account for it. Dogs are kept till they grow old, while pigs are as a rule slaughtered young. The former, therefore, have time to continue the action of the causes which induce cancer, while the latter have not. If, however, pigs lived to old age, I think it likely that, from their greedy and unclean food-habits, they would develop cancer quite as much or more than dogs—unless, indeed, they are immune to cancer, as some animals are to some diseases; but I do not think this likely in the present instance.

But to continue our inquiry into the causes of cancer as connected with the food habits of the

people. The next point which arises is that the causes, whatever they are, affect women more than they do men. In the years 1896-1900, 126,206 persons died of cancer in England and Wales; and of these 49,533 were males, and 76,673 females. The males were to the females in the proportion of two to 3·1. There were almost exactly half as many more women as men. But it is found that the proportion of males who die of cancer now, as compared with females a generation ago, is greater than it used to be. In the three years 1860-2, there died 6536 males of cancer, as against 14,963 females, which gives a proportion of two males to every 4·6 females. In the two years 1868-9, the numbers were 5676 females and 12,578 females, or in the proportion of two males for every 4·4 females. The proportion of males was smaller than it is now, and we desire, if possible, to find out the reason of that. First of all, we might ask the question, why should women suffer from cancer at a greater ratio than men? Is it because of sex or because of habit? The writer believes that the cause is rather habit than sex, as will be said later. But the present question is this: Seeing that women, for whatever reasons, suffer from cancer more than men, how is it that the preponderance of the proportion of female cancer is diminishing? Probably, the writer suggests, because the habits of men are approximating more in character to those of women than was formerly the case. And indeed this seems to be so. The habits of the English

race are altering, and rapidly. Under the commercial and industrial policy pursued by the people of the United Kingdom for the last hundred years (free trade was only an incident in that policy), the English race is being more and more congregated in towns, and there is much less difference than there used to be in the habits and occupations of the two sexes. It is not now as it used to be, when women performed the indoor occupations of life, and men did the more laborious out of door work. Both sexes are now doing indoor work, and, more important than that, are falling quickly into similar general habits, and more particularly into similar food habits. If women still perform the duties appertaining to the home, many of them also perform work strictly comparable with the lighter occupation now followed by men in offices, warehouses, manufactories, and mills, and women also work in mills, shops and offices much more than they used to do, so that in many respects the habits of the lives led by the two sexes are identical. Men still, of course, do the out of door work of railway building, road making, sewerage, drainage, waterworks, iron production in all its branches, and heavy works of all kinds; but there has been a very great diminution of late years in the number of men employed upon the land. It is to be hoped that this is a passing phase of things, but unquestionably for many years, the land of the country being more and more laid down in grass, and the habits of men and women becoming more alike, fewer and fewer men

have been employed upon its cultivation. When, therefore, the habits of the two sexes so much approximate to one another as regards occupation and food habits as has been the case of late years, it is not surprising that they should be found to be approximating to one another as regards the diseases from which they suffer. Neither sex now works very hard. Under the influence of a rapidly extending humanitarianism, legislation has been very active for some years in shortening the hours of labour in industrial work. But the work of women—what may be termed their natural work at home—is longer continued and more monotonous, and can be much less easily controlled than can that of men. Their work at home with the children, superintending and often performing with their own hands multifarious household duties which can hardly be regulated, and which never seem to come to an end, is certainly trying, and often even laborious, if in nothing else, then in respect at least of the very long hours over which it is extended, much longer for the most part than in the case of men. Add to this that the introduction of machinery has greatly lightened the work of men, and has so far approximated it in character to that done by women, a greater similarity now obtaining between the kinds of work done by the two sexes than used to be the case ; and we shall see some considerable reason to understand how the diseases of the sexes may resemble one another.

But there is one notable respect in which the

habits of women differ from those of men. They eat oftener. They do not eat more, probably, than men, but they eat oftener. It is not uncommon to find them breakfasting about 8 a.m., having some lunch of tea and bread and butter at 10-30 or 11 a.m., having dinner about 12-30 or one, tea at 4-30, and supper at 8 or 9 p.m. Further, if the kind of food they take is inquired into, it is found to consist (besides the chocolates and sweets which women seem to be constantly taking), largely of tea and bread and butter, mostly white bread, from which some of the natural constituents have been removed, rather than brown. In fact, at each of the five meals (or four, if the forenoon lunch is omitted), bread is eaten, very often with jam. Food is taken too often, and the kind of food is mostly bread and sugar. It must be very unusual to find anyone, man or woman, who eats meat five times a day, or even four times a day. To take meat even three times a day is rather uncommon, and I have never yet met man or woman who took it oftener. If meat were taken as often as bread, five times a day, or four times a day, no doubt it would set up other forms of illness. It is an interesting and also a very serious question, whether the frequent slight ailments of women, their headaches, their back-aches, neuralgias, rheumatism, frequent colds, and influenzas, are not in great part to be attributed to their food habits. Many of their ailments seem to be due to habit rather than to sex. How frequently and for how long they

suffer from dyspepsia, and indigestion, and heartburn; and what a large proportion even of the younger ones have lost their teeth from this cause. Very often, indeed, even the first set of teeth in children are soft and decaying, and no sooner does the second set of teeth appear than they are found to be decaying also. There is even comparatively little difference between the sexes in this respect, and the little boy, as well as the little girl, is apt to awake, as indeed they do, early in the morning, with the breath offensive and the mouth fetid from indigestion commencing thus early in life. Formerly the habits of the sexes diverged somewhat early in life. Boys went to work in the fields, while the girls stayed at home or went into service. On the whole the boys, and afterwards the young men, did harder and more laborious work than the girls, and afterwards the young women. And, besides this, the boys and youths, coming home at regular stated times for meals, kept on the whole freer from indigestion than the girls who, remaining at home and feeding at other people's cost, got into the way of taking more meals and at shorter intervals than the members of the other sex. The same causes acting now in the case of domestic servants are the chief factors in producing the frequent (I had almost said constant) illnesses suffered by this portion of the community, and attributed by them to any and every cause but the real one. Nowadays, however, both sexes go to school, and after 11 or 12 years of age, to the factory, and their habits are

almost precisely similar. When adult life is reached, and particularly after marriage, the habits may diverge again a good deal for some years; but very often both the man and the woman live similarly, perform the same duties, and follow the same avocations. And, in respect of health and sickness, the same kinds of illnesses appear to overtake them. But after the arrival of children, the kinds of work done by the sexes are again separate, and the women, staying at home to mind the children and the house (although in too many cases women's work is not so natural as this) fall into habits somewhat different from the men, and, finding themselves frequently suffering from indigestion, have recourse to frequent small meals of bread and tea, which no doubt often relieve them immediately, but mediately or remotely are too apt to aggravate the dyspepsia they were taken to relieve. After a period, longer or shorter, of experiences of this sort, and after suffering from a variety of ailments like those referred to, or perhaps quite as often from the frequent recurrence of some single ailment, like headache, or neuralgia, or influenza, or "bilious attacks," or rheumatism, or a constant feeling of fatigue, they fall into long continued or chronic ill-health, and after some years are found to be suffering from cancer. If the other and more trifling ailments had been properly grappled with; if in particular longer intervals had been allowed between the meals, the indigestion and mal-assimilation, having been prevented by this

means, and if the food products in their fermenting and ill-digested condition had been prevented from entering the blood and poisoning it, no doubt the occurrence of later and graver disease would either have been obviated or postponed. The indefinite postponement of illness is equivalent to its prevention. No doubt some sort of illness is bound to overtake humanity, however physiologically or rationally it lives, for we can hardly reach the perfection of health; but the more rationally we live, the fewer of these illnesses will be our portion. On the other hand, the general anxiousness of the times in which we live, and the interpolation of large numbers of small meals, habits affecting all classes of society, and affecting men to a larger extent now than formerly—all these causes are having a marked influence on our population, and are acting as serious deteriorators of health, and among other things, there can be little doubt, leading to the large increase of cancer. How this condition of things is brought about by improper feeding, and how the products of assimilation and of mal-assimilation of food are carried by the thoracic duct into the venous blood-current, I need not here repeat, it being sufficient to refer the reader to what has already been said on this subject in Chapter V. I may, however, add here (and it will, I think, help to shew that the view I am advocating is not theoretic only, but has a real and even grim existence as an actual fact), some experiences I detailed formerly regarding the causes of cancer. I

saw, for instance, in the Bradford Royal Infirmary, a woman, aged 42 years, "suffering from a large infiltrating malignant growth from lips of cervix uteri." (House-surgeon's note.) I had never seen the woman before. There was no doubt about the diagnosis; but I said to the resident medical officer; "You will find, if you inquire, that that woman has lived largely on carbonaceous food." Now one has to be very sure of one's position before making such an observation to a critical young professional man, who would in the nature of things be glad to gain an advantage over a senior by proving that he was in the wrong. Had I been wrong, no doubt I should have heard more about it. However, my young friend inquired, and ingenuously wrote the following note, which I read on my next visit:—"Patient is fond of bread; never eats potatoes; eats rice; never liked meat; bad teeth." I saw with Dr. H. a man, æt 56, suffering from cancer of rectum. His diet had been bread and butter and tea for breakfast, sometimes oatmeal porridge. Dinner: meat, potatoes, bread, and rice, sago, or tapioca pudding, often Yorkshire pudding. Tea: bread and butter and tea. Supper: oatmeal porridge. The whole diet here is starchy, except the small piece of meat (viz.: two or three ozs.) at dinner. That starchy food, taken over a course of years, had fermented in his digestive tract—how often these sufferers say they have had a "poor stomach"—had poisoned his blood, loading it with waste matters, and had, I have no doubt, led to the

production of the cancer. In any case, four daily meals, even if they had not consisted to such an extent of carbonaceous materials, would have been at least one, and perhaps two, too many; and would probably have led to illness and to early death, whatever materials they had been composed of.

I asked the matron of our small cancer hospital to give at random the diet of the first three cancer cases she chose to question. Here is the result.

1. "Mrs. B., æt 55. Breakfast, 7-30: tea, bread, bacon or egg. 11-30: Cheese, bread, beer. 1-30: Meat, potatoes, vegetables" (green? A. R.), "milk or Yorkshire pudding. 5-30: Tea, bread, butter. 10: Meat and bread." Note that this woman had five meals a day. She had bacon or egg for breakfast, and meat twice daily, which is unusual. But she had bread four times a day, and if Yorkshire pudding be counted (it is really the same thing as bread) five times. But she lived to 55, and, I have no doubt, might have survived to 65 if she had had sense to keep to three meals, like, say, her breakfast, dinner, and tea, with proper intervals between them.

2. "Mrs. S., æt 69. 8 a.m.: Tea, bread, bacon or egg. No lunch. 12-30: Meat, potatoes, sometimes vegetables" (green? A. R.), "and potted meat. 4-30: Bread, butter, tea, and remains of dinner. 10 p.m.: Potatoes, milk, or porridge." I have not much to say about this case. The woman survived to 69, and it may be said she had

lived her day, and must be expected to die of something. I incline to think that, had she had one meal after dinner in place of two, she would have been healthier, and would probably have lived longer. But I give the case as I got it.

3. "Miss F., æt 44. 8 a.m.: Tea, bread and butter. No lunch. 12-30: Potted meat and bread taken at the mill. 4: Bread and the remains of dinner. 7: Tea, bread and butter. 9-30: Bread and beer." Bread here was taken five times a day, and once with beer. As bread takes five hours to digest, often longer, let any one imagine the state of fermentation that that poor woman's digestive tract must have been in day after day, for say twenty years, with "tea" following dinner in three and a half hours, another "tea" following the first in three hours more, and a third "tea" following the second in two and a half hours. The only meal which was allowed any approach to a reasonable time for its digestion was breakfast; and dinner followed that in four and a half hours, while five and a half hours would have been much better. No wonder the poor thing did not survive beyond 44 or 45 years.

These are cases of women in the humbler walks of life. But their well-to-do sisters are doing the same thing in a different way. One of them told me some time ago that she had tea and cake, or tea and bread and butter, at no less than three different houses in one afternoon, and all within two hours, or even an hour and a half of one

another. Well for her if the attack of indigestion which ensued on that series of indiscretions shall teach her more sense, and help her to prevent the irreparable damage which, in one form or other, probably enough the onset of cancer, is certain to ensue if she persists in so insane a course. And yet, how many women are living like this !

The statistics of the Board of Trade regarding the immense increase in the quantity of sugar imported into this country in recent years, appear to me to support the view I advocate regarding the causation of cancer. Of course, too much stress must not be laid upon them, because much of the imported sugar is used in the manufacture of articles again exported. Nevertheless, I have a general impression, corroborated by several considerations, that our people are consuming much more sugar in various forms than they did. Bread and jam, as we saw before, is one of the cheapest forms of food; but while bread and jam, taken once a day, may be quite wholesome, bread and jam taken four or five times a day is a sickly and even deadly diet.

Here is another case of cancer. "Mrs. L., æt 50. Breakfast, 8 a.m. : bread and butter and tea. Dinner, 12 : meat, potatoes, Yorkshire pudding, rice pudding, bread. 4-30 p.m. : bread, butter, tea. 8 p.m. : bread, butter, stout, or quaker oats, or oatmeal porridge." The perpetual fermentation of food so induced resulted in the production of a malignant ulceration of the mouth of the

uterus. Cases of this kind could be multiplied indefinitely, and they afford the kind of evidence (which any one who chooses can verify for himself) which has led me to the general conclusion that too many meals, and especially when they contain too large a proportion of the carbonaceous and fermenting foods, form a main part of the predisposing cause of cancer. And as the habits of men are now approximating more and more to those of women, in the materials and in the number of their meals, they are falling victims to the same terrible scourge. Sometimes one is almost reproached, on being compelled to come to the horrible conclusion that it is cancer from which a patient is suffering, to hear him say: "Well, but I have lived so regularly." Yes, they have lived regularly, but regularly in a wrong or physiologically unsound or vicious way. What can this result in but disease and early death?

But if these are the most important and the most potent causes of cancer, no doubt others are contributory. One special form of female cancer is called *morbus miseriæ.* Some poor women (not always, unfortunately, poor only in material wealth, although this form of cancer is commonest among the conventionally poor) being joined to husbands who are careless, or negligent, or inconsiderate, or brutal, and who squander their earnings, or who are perhaps too idle to have any, suffer in quiet and uncomplaining ways untold miseries lasting over years. Wives who are better

off can often defend themselves by finding resources in other directions than those of domestic duty; and in the pursuit of pleasure, or perhaps in works of philanthrophy, or of religion, solace themselves for the loss of the domestic happiness which they have given up everything to obtain. But the very poor have no such resources. Compelled, perhaps, to go out charring in order to find food for their children as well as themselves, or even for idle and dissolute husbands, and happiness being entirely unknown to them, some of these women are found early to develope a special form of cancer, termed in such cases the disease of misery. No doubt, some proportion of cancer may be contributed to in this way, especially as the food habits of such poor women are generally those which have just been described, I doubt, however, if any very large proportion of the cases are caused wholly or in part in this way. Cancer, at any rate, although it deals desolation among the poor, does not spare the rich; and in the case of most of the latter, as well as of most of the middle-class women, it would be out of place to speak of the cancer that occurs among them as a *morbus miseriæ*. As regards anxiety, again, as a cause or a contributory cause of cancer, it is not easy to point to much evidence. Both sexes suffer from the anxieties to which humanity is subject, and each sex, no doubt, has its own special troubles. If, however, one might venture to hazard an opinion, men seem to suffer from anxiety rather more than women, many of whom, happily

for themselves, appear to accept their troubles rather in the spirit in which children meet theirs, in a matter-of-fact way and uncomplainingly, as if they belonged to the nature of things and to the inevitable. Men, on the other hand, are more apt to chafe, and fret, and grumble, appearing also less able to derive comfort from the consolations of religion, which act as such a calmative to the women. Suicide, at any rate, is much less common, as is the despair which leads to it, among women than among men. It does not, therefore, seem easy to say how far anxiety enters into the causation of cancer. That it is often a contributory cause I do not doubt.

Some other conditions have been said to induce cancer. Some evidence has lately been brought forward regarding what have been called cancer houses, it being alleged that, in some houses, tenant after tenant has succumbed to this disease. A great fallacy here seems to be that, even if this is so, no special inquiry has been directed into the habits of the successive residents who suffered in these ways. Obviously, much, almost everything in fact, depends on this inquiry. By convention most of us live after the prevailing fashion of the time: we live as the majority live; and if the habits of the majority are unhealthy, it may well be the case that a succession of tenants occupying a particular house, being at an age suitable for the development of cancer, might have so lived as to induce it; or the effects of the causes acting in

the previous parts of their lives might have culminated during the periods of their tenancies, the occupation of the house being quite accidental, while the influence of the habits of the tenants was the all important factor.

It has also been said that low-lying districts, wet and swampy, help in inducing cancer in their residents. This is quite probable. Damp is a contributory cause of most illnesses; and it may be admitted to be a contributory cause of cancer also without any damage to the theory that far and away the most important cause is wrong living, and particularly the dyspepsia that is induced by the taking over a long period of time of too frequent meals. Alcohol does not seem to have much influence in the causation of cancer. At least, men take alcohol to a much greater extent than women; and men do not even yet suffer from cancer so much as women. At the most the influence of alcohol might be admitted as a contributory cause of cancer. Damp and alcohol no doubt aggravate such illnesses as bronchitis, pneumonia, and rheumatism, and yet we find many cases of each of these diseases among teetotallers, where, at any rate, one of these causes cannot be an efficient factor.

Such being, then, the predisposing causes and such being some of the exciting and contributory causes of cancer, and since so little prospect remains of effecting a cure when the disease has become established, the great question of prevention rises

before us. It has been said that prevention is easy, and in the sense already explained this statement may be repeated, it being remembered that what is meant is that not theoretical but practical prevention, that is, a very considerable diminution of cancer, can easily be attained. As much, I have no doubt, or more, might be effected towards the prevention of cancer as has already been effected towards the prevention of consumption and the fevers, both of which, as we have seen, cause many more deaths than cancer. Cancer might easily be reduced by 40 or 50 per cent. from causing a mortality of 700 per million per annum down to 425 or 350. If the causation is known, the question of prevention becomes quite simple. If taking too many meals is the main cause of cancer, taking fewer meals ought to have some influence in preventing it, and so in checking its course. I may say here that since I first stated the opinion that such was the chief cause of cancer, a great change has become evident in medical views. A clever medical critic of the first edition of Air, Food, and Excercises, "smiled" when he referred to it—I was going to say discussed it—but he did not discuss it. He only "smiled." Now, Dr. Braithwaite says in so many words that cancer is caused by too much food. So does Sir W. Mitchell Banks, who thinks, however, that meat is the chief cause. It may be so in the cases which have come under his notice; but that is not my experience, for I have found a very large number, in fact the great

majority of the women whom I have interrogated on the subject, say that they did not like meat and did not indulge in it much. But whether it is meat or bread is an accident. The principle is that too much food is the cause, the chief cause, I mean; and in that opinion we are at one. I hope, therefore, that the critic will cease to smile, and will preferably consider with candour a view which has some weight of authority regarding the causation of a disease from which the people are suffering and dying in thousands every year even in our own country, and in millions over the whole of the known world; and which none of us can cure. A smiling professional critic and a suffering and dying people is not a spectacle which commends itself to my sense of fitness.

Dr. Braithwaite's statement regarding overfeeding as being a cause of cancer is introduced, by the way, in a pamphlet which he has written to shew that an excess of salt in the diet is the chief cause of the disease. I have read the pamphlet twice, and the argument appears, I regret to say, weak in the extreme. His theory is that "excess of salt in the diet is one of four factors which cause the disease." But he does not define what is "excess of salt." He is not even certain in his own mind, for he remarks: "It is a debatable question whether the salt must be in great excess." The idea that salt was the "essential factor" originated in his mind by noticing that cancer of the uterus was seldom or never met with among Jewesses.

But the British Medical Journal says in the number of 5th October, 1901 : "As a matter of fact cancer is extremely and increasingly prevalent among the Jews." So the fact is denied. Then he states that excessive meat eating is a cause of cancer (which no doubt it may be), and then he goes on to assume that excess of meat means an excess of salt (which it may or may not do). Now it does not follow that those who take much meat take also much salt, and it does not follow, either, that the flesh eaten contains much salt, because the flesh of young animals does not, as a rule.

"The second point," he continues, "which led me to the idea about salt is much more certain, and it is this : In vomiting from cancer of the stomach, Vander Velden found that there is almost always no free hydrochloric acid in the vomit, whereas in all other forms of vomiting it is present. Hydrochloric acid is derived from salt, so that there appears to be some connection between salt and the disease." But surely if this argument proved anything it would go to shew that absence of salt and not excess of it might be a cause of cancer. There seems indeed to be a good deal of force in the arguments of Mr. C. Godfrey Gümpel that deficiency of salt and not excess of it is a potent cause of cancer. He has saved me the labour of criticising the opposite view, and indeed I have quoted him rather than expressed in my own words the opposition to Dr. Braithwaite's view, which was stirred up in my mind by reading his pamphlet.

Mr. Gümpel's case for the defect of salt as a cause of cancer, is so strong that it seems to me it must be admitted as to a certain extent proved. I still, however, think that excess of food is the chief factor, and that the accidental form which this takes is usually an excess of starch and sugar. The taste for these foods is usually accompanied by a distaste for salt, and I think usually by the habit of taking little or none. I think, therefore, from this consideration, and from a general review of the whole question, that we ought to consider deficiency of salt as a contributory cause, and probably an important contributory cause of cancer. Mr. Gümpel quotes the statements of physiologists that the blood cannot exist with less than 2·5 parts of salt in 1000 parts, and that it contains from this proportion up to six parts per 1000 in varying conditions. In order to keep the blood healthy, therefore, I am disposed to recommend those who wish to be free from cancer (and other diseases) by simple means, to take a cupful of water morning and evening, containing a pinch of salt in each, just enough to make the water pleasant. A saltspoonful of salt also in the morning coffee is a very palatable addition, much pleasanter, when one gets used to it, than sugar. By this means one may prevent the too great diminution of salt in the blood, and the too great tendency to putrefactive change which such a diminution may imply, and so prevent some of the conditions which favour the growth of cancer.

If, then, as I think, and as I have attempted to shew, too much food is the cause of cancer, if taking too many meals is the cause, if eating too much meat is the cause, or if too much bread and too much sugar is the cause, combined with too little salt, let us advise the people to take less food, to take fewer meals, to take less meat, or to take less bread and sugar. Plainly there is sufficient agreement among authorities to enable us to concur in offering certain advice to the people. I need not repeat here the statements made in a previous chapter, regarding the time required for digestion, or regarding the intervals which ought to elapse between meals. I think I may say, without fear of contradiction, that two meals a day is sufficient for most people, and especially that they are sufficient for townspeople, who now form the large majority of our population. My advice, therefore, to those townspeople who wish to avoid cancer is to eat a mixed diet not oftener than twice a day, at say seven or eight hours' interval, to eat, for instance, at 9 a.m. and 4 or 5 p.m., or at 12 noon and 7 or 8 p.m., according to the circumstances of their lives. And if in quantity they do not exceed about two pints of fluid a day, and about one ounce daily of food for every ten pounds avoirdupois of their body weight, or say, perhaps, one and a half ounce, I am sure there would soon be much less cancer to chronicle in our midst. The hot drink morning and evening, say with a small pinch of salt in it to make it pleasant, and to add

to the blood a necessary ingredient, would be a suitable part of the diet, and would act in the way formerly suggested. It will be noticed that this advice is very much the same as that given for the preservation of health generally. It is the simplicity of the advice that is at once its strength perhaps and its weakness. It is its strength because it is in keeping with the view that disease is one in all its varied and various phases, and that its chief cause is mal-assimilation of food from the ingestion of it too often and too much. And it is its weakness because people expect to be recommended to do some great thing, some obscure thing, some difficult thing, in order to be freed from a disease hitherto so obscure in their minds as cancer; and because they are apt to be impatient when they are advised simply to eat twice a day, or once a day, after the age of say 55 years. Also they do not like the suggestion that they are or have been too fond of the pleasures of the table. But common sense and candour ought to be great enough to overcome these objections. Self-restraint and self-government are not sufficiently commended to our favourable consideration at the present time. I think it would be well for us if we thought more of them; and on the evidence, as it presents itself to me, I cannot fail to press this advice with what energy I can muster, feeling, as I do so strongly, how much benefit we might as a people reap from following it, how much suffering we might prevent, how much

happiness we might attain, both for ourselves and for our households, and how much more useful and complete as well as more happy we might make our lives. *Καλὸν γὰρ τὸ ἆθλον καὶ ἡ ἐλπὶς μεγάλη.* For fair is the prize and great the hope of attaining it.

CHAPTER XII.

Some General Observations; and some Cases illustrative of the General Conclusion of the Writer regarding the Chief Cause of Disease in general, and of its Cure by Diet.

I HAVE now said *in principle* pretty nearly all I have to say regarding the causation, or the chief cause (for causes are always multiple, and the causes of disease form no exception to this rule), of the great mass of diseases which afflict humanity. *In detail*, of course, each case of disease is unique to the person affected by it; but the medical man, while he feels that patients very often shew a certain amount of resentment at having their cases referred to a class, or in being classified themselves, and while he would, perhaps, if he could, much prefer to let them continue to magnify their own importance by encouraging them to think that such a case as theirs was never known before, is not unfortunately always able to do so, and insensibly refers the case, both as to its causation and classification, to one of several groups which he has in his mind. In fact,

if he did not or could not do so, there would be no possibility of science or of philosophy in medicine. In this chapter I propose to make some general remarks, and to offer some observations which the reader will, I hope, be more or less prepared for, since the general idea underlying them must by this time be familiar to him. And, besides these general remarks, I have thought that it would be useful to detail some further instances of the application to the treatment of disease of the main proposition laid down. That proposition is, that disease is far oftener due to mal-assimilation of food than to any other one cause, and that, of the various forms of mal-assimilation of food, excess of consumption of good food is a far commoner cause of disease than either deficiency or badness of food. The description of some additional cases in which the application of this proposition or principle has not only cured long-continued or recurrent illnesses, but has also prevented their recurrence, will be of interest to the reader, and, still more, of value to sufferers. For, as has been said now more than once, while a knowledge of causes may not enter one's mind, or may not be necessary in the treatment of illness, such knowledge is absolutely necessary to prevention. And further, if the application of theories as to causation enables us to prevent the recurrence of a given illness, especially if a patient has often suffered from it before, our very success acts as a piece of additional evidence for the truth of the theory, which, on

general principles, and as a deduction from our observations, we had been led to adopt. The interest of the public is no doubt summed up in the statement that prevention is better than cure. For the doctor, on the other hand, cure is much better than prevention. A trader is glad when the prices of commodities rise; the public when they fall. Yet a wise trader often manages to reconcile his own with the public interest by the avoidance of cornering and other measures calculated to unduly conflict with it. As to the prevention of disease, the medical man, I fear, has no chance of ever making himself unnecessary. Do as we will, and interpret nature and her laws as we may, there will always, I am afraid, be a residue of illness which our efforts will fail to prevent; although I have no doubt that the residue would be much smaller if the methods of prevention proposed were more likely to obtain not only public sanction, but active public support. The clergy and the lawyers are in much the same condition. The numbers of the persons filling both of these honourable professions might be greatly diminished with advantage both to the public and to themselves; but there does not seem to be any immediate prospect of this result being attained, since the methods of self-control and of the pursuit of justice, by which alone such a diminution could be attained, do not attract the majority, but only a small minority of mankind. If, however, a medical man can do anything in the direction of advising people how to live so as to

prevent disease, he will have achieved something worth attaining; and at all times the medical profession has advised its clients up to its lights in this direction, as indeed most notably have the clergy, and to some extent even the lawyers.

Under the terms, predisposing causes and exciting causes, as also under the notions of cause and occasion (αἰτία and πρόφασις, as Hippocrates has it), I have dealt with these ideas to some extent already. What is called Etiology, or Aetiology, that is, causation, is too often in medicine nothing more than a *Prophasiology, i.e*, the so-called causes of disease are very frequently only its occasions. Perhaps a few more instances of the kind of cases which justify this statement may be useful here. A young man will come to the doctor, say, with a number of suppurating pimples on the side of his neck, just under the angle of the jaw, and will say that friction by the collar-edge has been the cause of these. Or he may perhaps have one large similar suppurating point (a boil), or, if older, even such a place with three different heads on the back of his neck (a carbuncle), just where a stud has been pressing and rubbing it. Or a person may suffer from corns on the toes, which he may attribute to the pressure of too tight boots; or he may suffer from chilblains, which he attributes to frosty weather. Or a woman may have a cancerous growth develope in her breast through a slight blow accidentally given her by her own child, all unconscious and even

incapable of understanding the damage he was doing. And I have already mentioned the case of an old gentleman of 87 years of age, who, being knocked down by a bicyclist, suffered from an attack of gout. Now all these alleged causes are instances of prophasiology rather than of etiology —they are samples of occasions rather than causes; they are instances of exciting causes rather than of predisposing causes—instances of the action of recent causes acting only once or seldom, rather than of the action of remote causes acting frequently and with cumulative effect.

Of course, all these alleged causes were part of the real causes, for without these particular prophasiologies, none of the illnesses mentioned would have occurred just when, and where, and how they did. Without the fraying and rubbing of the collar or stud, neither the pimples nor the carbuncle would have occurred just then and there, although, no doubt, any other slight cause or occasion might have set them up. Without the frosty weather the chill-blains would, or might probably, not have appeared just then. Without the tight boots there would probably have been no corns; without the blow cancer would probably not have occurred just where and when it did; and without the bicycle injury there might have been no attack of gout at that time. The reader must not suppose that this is a mere theoretic or academic discussion without any practical consequences, or that its consideration is like the school-men's

question, "How many archangels could dance on the edge of a razor, or on the point of a needle?" It is on the contrary full of the grimmest practical consequences. Without the predisposition none of these prophasiologies could have been converted into aetiologies; none of these occasions could have been the causes of the illnesses more or less serious which followed on them. And to a very much larger extent than is commonly thought, *we make our own predisposition;* that is the important matter. It is not the rubbing by the collar; it is not the frost; it is not the tight boots; it is not the blow; it is not the bicycle accident; but it is in all these cases (as in a thousand others which will occur to the reader—I could multiply them indefinitely, and, in fact, might never be done) the way in which these different people have lived before these events occurred, and it is in particular their previous food habits more than anything else, which have been the real causes why they suffered from their illnesses. If we did not all of us load our blood, and overload our blood, and that habitually, with more food and drink than we require, the slight causes mentioned above would have failed to induce the illnesses mentioned. And I emphasize food more than drink, not, as I have said before, because I think alcoholic drink an unimportant cause of illness, but because I know food to be so much more important a cause, that while on the whole only comparatively few of us suffer from the effects of too much alcoholic drink, nearly all of us,

without exception, suffer from an excess of food. The way in which the foundation then is laid in the body for the occurrence of these illnesses from causes so slight is the way in which disease begins in the body, that is its first step is usually indigestion in some form. And, beginning with indigestion, which is nearly always, or at least, very often, caused by too many meals taken too close together in time, many varieties of illness are apt to occur. It will be useful to examine these a little more. There seem to be two great lines of the development or evolution of disease. In one, the sequence of events is indigestion, heart-burn, acidity, the occurrence of watery blebs or blisters on the lips or tongue, sore throat (tonsillitis), acne of the skin, rheumatism (initis, I have ventured to call it—congestion of connective tissue, generally lymph-congestion rather than blood-congestion), constipation, bronchitis and broncho-pneumonia, pneumonia itself, scanty high-coloured urination often accompanied by a heavy deposit on standing, insomnia, eczema, and apoplexy or cancer. In the other we have indigestion, fulness and weight after eating, faintness, relieved immediately by frequent eating, and remotely aggravated by the same, enlargement of glands in the neck, the watery blebs on the lips mentioned above, free urination without deposit or precipitate, tendency to free perspiration or sweating, the occurrence perhaps of disease in a joint such as the knee, hip, elbow or ankle, anæmia (triphthæmia, or catatribæmia rather it

should be called), pallour and attenuation, feeling of general or frequent fatigue, *peliosis*, or proneness to become black and blue on receipt of very slight or unremembered injuries, flushing, followed by coldness, clamminess of hands, rheumatism, diarrhœa, pleurisy and tuberculosis. Of course, these groups of illnesses, one culminating in cancer, and the other in tuberculosis, are not definitely demarcated off from one another; but still, I think, perhaps, are more or less fairly defined. However, the important thing is that they both commence with indigestion, and the inference therefore rises before our minds that if this could but be dealt with, all the other evils, or most of them at least, could be obviated. Let us look for a little longer at the course of things. The function of food being to make blood; when indigestion occurs, the blood is furnished with ill-made, ill-digested stuff. Some of this gets stopped at the capillary circulation, or at the lymph spaces beyond, the consequence being that the lymph ducts and lymph glands become inflamed and may even suppurate, causing those so-called scrophulous affections so common among children and young persons. The word scrophula, or scrofula, is itself interesting, and of somewhat doubtful etymology. ϛκόροδον or ϛκόροφον is the Greek name for a species of onion, which the enlarged glands of the neck were fancifully thought to resemble. By the omission of the ο, the word got altered to σκρόφον, hence σκρόφυλον, and hence scrofula came to be that

form of disease in which the glands of the neck become enlarged and infiltrated, as if onions were dotted along it. Of late years it has been found that the tubercle-bacillus is constantly or at least very often associated with the suppuration which so frequently accompanies such gland inflammation; and hence the modern view of tuberculosis is that it "is an infectious disease due to the presence in some part of the body of the *bacillus tuberculosis.*" I do not think this is a proper statement or definition, and think that it would have been better to define tuberculosis as that state of the body which favours the growth in it or in some part of it of the *bacillus tuberculosis.* We all, it seems to me, so ubiquitous are the germs of tubercle, have them in us, we are swallowing them in milk and meat, and inhaling them in the form of dried sputum all the time. We cannot avoid doing so if we are to mix with our fellows, and do the work we were intended to do in life. It is all very well to take all the means we can to avoid infection, or "to keep away from it," as we are so often recommended to do. But evidently beyond a certain (or uncertain) point it is not possible to do this. How can a husband keep away from his wife if she has an infectious illness? How can a wife keep away from her husband, or still more, how can a mother and father keep away from the children in such circumstances? Is it even morally right that they should do so? Or should a man desert his friend in such circumstances? Is it not rather cowardly to do so?

In the case of parents, or at least of mothers, it is hopeless (as a rule) to inculcate or expect it. And when it seems possible by proper living to get into such a state of body and mind as that we need not fear the pestilence that stalketh in darkness, nor the destruction that wasteth at noonday, is it not far more to the purpose for the medical profession to attempt to educate the public and shew them how to make themselves immune to infection, than to fill them with the fear and terror which induces them to try to "keep away from it?" Of course there is reason in all things, and I am not advocating recklessness in the exposure of ourselves to infection, although I do think the present attitude of the public in reference thereto is often craven and cowardly. A little more consideration of the subject, and a little more attempt to put government and restraint upon ourselves and on our appetites, would soon bring about a much healthier and happier and more courageous public opinion on this subject of infection.

As regards the cure of particular diseases by diet, one of the first conclusions to which I came was that bronchitis, as it appeared specially to be caused by errors in diet, was particularly amenable to this form of treatment. This was at that time rather a revolutionary idea to my mind, because I had thought, in common with all the world, I suppose, that bronchitis and broncho-pneumonia, at least, were forms of "taking cold," and were due to the action of "cold" for the most part, to exposure

to wind, cold, wetness, "drafts," &c. Indeed, it seemed likely *a priori,* so to say, that bronchitis would be due to the action of cold air inspired, or wet air, or "draft," since the function of the lungs is performed through inspiration and expiration, and any disturbance of the balance of health in those organs would very likely be attributable to some change occurring between the body and its relation to air. An upset of digestion, on the other hand, might perhaps be readily enough attributable to some error in diet, because the function of digestion is to deal with food and to convert it into blood and lymph. This, I take it, was the form taken by the unconscious reasoning adopted by one's mind, or would have been, if the process had been put into conscious thought. However, there was this amount of possibility that this newly suggested mode of causation might be the real one that I was taught as a student, and medical men had for two generations expounded the idea that asthma at any rate was caused to some extent by errors in diet; and if asthma, why not bronchitis also? After all, asthma is only a special form of bronchitis, the form in which as a rule some spasmodic contraction takes place in the transverse muscular fibres of the bronchial tubes, preventing the patient from getting rid of the air which he has forcibly inspired, and which, remaining in his air-cells loaded with carbonic acid and other effete materials, which ought to be eliminated from the body, poisons the patient more or less, and leads through

the continually repeated and forcible inspirations to dilatation of his air-cells, and to other disease-changes in the breathing organs. The patient thinks that he cannot get air to breathe, while in point of fact he cannot get rid of the air he has breathed. He continues making rapidly repeated forcible *inspirations*, while he ought to be making long and repeated *expirations*, so as to rid his respiratory apparatus of the waste-laden air which is oppressing him so sorely. But in treating asthma, my teachers and the medical profession generally had repeatedly recommended that patients should at least abstain from taking supper, and the good effects of pursuing this line of treatment were generally admitted. I did not at that time know (say 20 to 25 years ago, for it was not yesterday that more or less vaguely this idea of the supreme influence of food in causing disease began to possess my mind), I did not then know that the best way to treat severe asthma is to put the patient either on nothing (to make him fast), or on say a pint of milk mixed with water each 24 hours, with perhaps half a pint of mutton tea or chicken tea, and to keep him to this for 10, 12, 14, 16, or 20, or even more days, until his breathing becomes sufficiently easy to allow him to lie down, after which, cautious increase of the diet may be allowed. From forbidding supper, however, in the treatment of asthma, a quite accepted method of treatment, to diminishing food in other forms of bronchitis was not, or ought not to have been, a very difficult step. It was, however,

in fact, rather difficult. Having suffered very much from bronchitis and frequently recurring colds in my own person, and having also suffered from asthma so severely as to be unable to lie down for nights together, and having also had change of air prescribed without much or without more than very temporary benefit, I felt that something else ought to be done, and I thought also that in the nature of things and on general principles, it ought to be possible to do something for a man about 40 years of age, who, if not robust, had yet always been fit for any work that had come his way. About this time I read Dr. Salisbury's book, and was by it, I think to some extent confused in mind between the effects of too much food and of too much starch and sugar in causing disease, and particularly in causing bronchitis. As is well known, Dr. Salisbury recommends a diet of beef and hot water in the cure of this and other diseases, and contends that persons can live indefinitely on beef and water alone without the need of any other food. In my case the effects were good so far as the relief to the asthma and the bronchitis was concerned, and even the rheumatism from which I had suffered (and which was, I now know, only another phase or form in which the poisoning of my blood by too much food expressed itself), improved greatly also. But it was some time before I saw that the relief experienced from these ailments was not positively due to the beef I was living on so much as it was negatively due to the diminution of my food, which,

in following the suggestions of his book, I had unconsciously made. For I by no means took the same quantity of beef that I had formerly taken of food in mixed meals, repeated three, four, or five times a day, as had been my habit. In fact, 4 ozs. of beef taken three times a day is a very restricted diet, and it was the restriction of the diet, I now see, which did me good; and I believe I should have received as much benefit by a restriction to 4 ozs. of ordinary mixed food three times a day, or to 6 ozs. of the same twice a day, as I got from the beef and hot water; and I believe further that I should not have experienced the distressing weakness which stopping all starch and sugar induced in me for a long time afterwards. Studying Dr. Salisbury's work and following its precepts did much good; but it unfortunately for a time confused in one's mind the essentially bad effects of taking too much food (polysiteism and pollakisiteism) with the accidentally bad effects of taking too much starch and sugar (poly-amylism and poly-glycism, and pollaki-amylism and pollaki-glycism). But the confusion did not last long, for it speedily appeared that to recommend say 3 lbs. of beef to be taken in a day would be as great an error as to recommend 3 or 4 lbs. weight of mixed diet, as some physiologists calmly do now. Three pounds weight of beef taken daily is too much food for the average man, and must translate itself into some form of illness, just as much as ingesting 3 or 4 lbs. weight of ordinary mixed diet must do, the

only difference being that too much meat would probably cause one sort of illness (gout, *e.g.*?) while too much mixed diet, and particularly, as in the general case, too much starch and sugar, would probably cause illness of some other sort (bronchitis, *e.g.*, or asthma?)

These being the ideas in my mind, I had naturally, in the course of practice, many opportunities of putting them to practical tests. In the first edition of *Air, Food, and Exercises,* I described the case of a well-built woman, 52 years of age, who came to me, saying she had suffered for twelve years from what her medical advisers called bronchitis and asthma. She said the attacks were very severe, and were coming on at continually diminishing intervals, and with continually increasing severity. Attacks which used to be three or four months apart were appearing at eight weeks' and six weeks', and even three or four weeks' intervals, and she was becoming worse and worse. The question was, could anything be done for her? This seemed an unpromising problem to attempt to solve in the case of a woman no longer young, who had suffered for so long a time. She was not very ill, however, at the time, as she was able to present herself for treatment. On examination she was found to have catarrh of the larynx, trachea and bronchial tubes, there being sibilus and rhonchus audible; and I thought that there were some evidences also of emphysaema or over-distention of the air-cells in the lungs. The question was, what

were the causes of the recurring attacks? Unless the action of these causes could be obviated, other attacks would certainly take place. On inquiry, however, it was quite plain what the causes were. Bread and butter and tea for breakfast at 8 a.m.; meat, potatoes, Yorkshire pudding, rice pudding or jam pudding, with bread, for dinner at 1-0; tea, with bread, butter, and jam at 4-30 or 5; and bread and milk or boiled oatmeal for supper at 8-0, constituted the woman's ordinary diet, and, in my opinion, accounted sufficiently for her attacks. At any rate, considering that she was taking far more carbonaceous food than she was assimilating, and believing that the laryngo-tracheo-bronchial mucous membrane was congested and irritated by the deposition or precipitation into it from the blood of some of the surplus material which had found its way into the blood by such excess, I advised an alteration of the diet to three meals a day, and that she should take bread only twice, and jam and sugar not at all. If she wanted any supper she was to have simply a glass of milk, not bread and milk, nor any oatmeal porridge. She came at weekly intervals for a few weeks. It was a little difficult to keep her confidence, because two or three of her previous medical advisers had said that the food taken had no influence on the bronchitis. However, she persevered, losing weight under the treatment, but losing also the congestion of her pulmonary mucous membrane. After the lapse of about five weeks she informed me that,

judging from her former experience, she might expect to have a new attack; "but," she added, "I am not going to have one." She has not had any severe attack of bronchitis since. So much for the history of this case up till the publication of the first edition of this book. On May 8th, 1902, this woman, then 63 years of age, came to me suffering from rheumatism of both knees, or well marked initis as I have called it, also from varicose veins in the legs and thighs. The bronchitis, she told me, was much better, and would be better still, she said, if she "minded her diet" as I advised her, but she said she got tired of it, &c. (unfortunately, humanity does get tired of self-control). This rheumatic and varicose condition has replaced the bronchitis, or it has supervened on it; but she has not required medical advice between November, 1893, and May, 1902. Arthritis, of right knee joint particularly, and periostitis of the right tibia with varicosity, have supervened on the bronchitis for which she consulted me about ten years before. I point out here that this sequence of ailments corroborates the general view I have been driven to take by the evidence, that disease is one (in this re-discovery I find I have been anticipated by Hippocrates, and the greatest of the early medical writers—how, indeed, could it be otherwise?) and also that bronchitis and rheumatism (*initis* or congestion of connective tissue) are different phases of the same thing, viz., an excess of material finding its way into the blood, mainly from indigestion.

Let us look at this case a little longer. In itself, of course, it is not much. It does not concern the life of a king or noble or merchant prince, but the principles underlying the sufferings of a working woman are the same exactly as determine the health or otherwise of all these important personages. It may be held that the history of the case proves nothing; that the recovery was a coincidence; and that the woman would have recovered, whatever she had done and whatever food she had taken. I do not, however, take this view. In view of the facts that she was not young, that she had suffered for so many years (no less than about a quarter of her life), and that the opinion that an excess (specially) of carbonaceous foods getting into the blood through the digestion produced material which was deposited as an inflammatory exudation in the pulmonary mucous membrane, is a physiologically correct opinion, and therefore that the excess of these foods was the main cause of the bronchitis—in view of all these considerations, I am compelled to the conclusion that the recovery was no mere coincidence, but was really the effect of the treatment. The critic may say—well, the bronchitis was cured, but the patient became rheumatic (she was somewhat rheumatic before, however), she was not much better, on the whole, as she changed one ailment for another. To this I should reply that she required no further medical treatment for ten years, and that at 63 years of age one may expect to suffer from

something. But I freely admit the force of the suggestion, which, I daresay, occurs to the reader as well as to the critic, viz., that it would probably have been better to advise her to take two meals daily rather than three. I quite think so myself, and the good effects of this advice in other similar cases makes me feel pretty confident on this point. I do not know if she would have been willing to follow the advice even if I had offered it. However, much further experience of the good effects of this advice has strongly confirmed my conviction of its value. I have already, at pp. 104-5, &c., related the case of a middle-aged man who put an end to the repeated attacks of broncho-pneumonia from which he had suffered by altering his diet. But another very interesting point in this man's case is that, whereas for several years consecutively he had had an attack of influenza in the spring, for some years past he has ceased to have such attacks; so that I have had the good fortune to prevent the recurrence of successive attacks of simple inflammation (broncho-pneumonia), and also of "fever" (influenza), by inducing a man to adopt the simple course of reducing the number of his meals from three or four to two daily. I have already mentioned the case of a man who had a long succession of colds, and also a succession of attacks of ague extending over thirty years, from the time of his leaving the West Indies, where he suffered greatly from malaria, but who for ten or twelve years now has neither had a bad cold nor a single

attack of ague, and this by the simple remedy of eating twice, and sometimes once, a day. Here, again, not only have inflammatory attacks of broncho-pneumonia been entirely prevented for many years, but also a fever like ague or malaria. But I think the most remarkable case I have ever had is the following :—An old lady of 72, feeble, thin, and attenuated, had suffered so much from repeated attacks of broncho-pneumonia that for twenty years she had been confined to her house, except that during the warmest weather in summer she sometimes ventured to walk for perhaps ten minutes at a time in front of her house in the sun. By following the simple advice of reducing her food to two small meals, one about 12 noon and one about 7 p.m. (both together not amounting to 12 ozs. of mixed diet daily), this old lady, two years after I had seen her, was able to visit friends at Clayton and Bingley, although for over twenty years she had been unable to venture on such a thing. I must say I was myself greatly impressed by the results of treatment in this case, much more effective and much more striking than I had ventured to hope for. This treatment is so valuable and so certain in bronchitis and broncho-pneumonia, and the evidence in favour of its efficacy is so strong, that I venture now to lay down the dogmatic position that the chief predisposing cause of bronchitis is an excess of food, and that in most cases the special form of the excess consists of such foods as bread, potatoes, puddings, and sugar. Of

course it does not follow that in order to get rid of bronchitis we ought to cease to take such foods, but it does certainly follow that we ought to diminish them. But bronchitis accounts for about a tenth of the total mortality of this country; and, therefore, the inference is that one-tenth part of all the fatal illness in England and Wales to-day is amenable to hopeful treatment; and further, that by proper dietetic management, bronchitis is preventible. This opinion is, of course, one fraught with very serious and very important consequences. If, however, we are in the presence of a physiologico-pathological law of the human organism, the principle can be put into operation by others. If it is, I make bold to venture the prediction that the law will be vindicated, and that bronchitis will not only be cured by the inculcation of a suitable diet and regimen, but that it will also be found possible by similar means to prevent it; to prevent it, that is, as a rule (for I suppose there must always be individual exceptions) between the ages of five and sixty-five years. I look on the case in which bronchitis of twenty years' standing was cured after seventy-two as too exceptional to allow us to expect so good a result at that time of life as a general rule—although this general view ought not to make one refrain from suggesting rational and possibly helpful treatment at any age, however advanced. The facts already mentioned that "old people bear abstinence well, and that they bear over-feeding badly," fill the medical adviser with

new hope in attempting to prolong the lives and relieve the sufferings of the aged.

I do not know how the narration of the following will strike the reader. The writer lives in Joplin, Mo., U.S.A. I have never seen either him or the acquaintance whose case he describes, and it is not likely that I ever shall see either of them, and as there are no relations between us except those of inacquaintance, reasonableness and goodwill, I think all other motives for making the narration may be put out of account. After stating that the writer of the letter to me had "read repeatedly and with very great benefit to himself" my first edition of "Air, Food and Exercises," and asking some questions about the new edition of the same, he continues :—

"If you will pardon me, I will give you a little incident showing the good your book did to another, and no doubt has been the means of prolonging a life. About one year ago I met a gentleman in Sumpter, State of Oregon, U.S.A., who complained of asthma, saying he had not been able for ten years to sleep in the ordinary position, but slept, bolstered up, sitting almost upright in his bed, not being able to lie down, on account of choking so much as to prevent sleep. Remembering your book, I ventured to inquire if he ate a good deal, and especially of carboniferous foods, and he replied in the affirmative. Having your book with me at the time, I offered to lend it to him, if he wished, and he gladly assented. As he told me, he read it through six times, realising that it fit his case exactly, and he at once put it into practice, and reduced his meals from three to two a day, and with immediate benefits of the most marked kind. He is over 60 years of age, and is a well educated man, and a mining engineer, and so has a good knowledge of chemistry, having devoted some 30 years to a study of the chemistry of rocks, metals, fuels,

and water, but never a thought did he ever before give to the chemistry of foods. Like a flash, he says, he realised the force of your position, and began to eat sparingly of carbonaceous foods. Within two weeks he lost, to his delight, some ten pounds of flesh, and found he could lie down and sleep like others, and soundly, and he was as happy a man as I ever saw.

In telling his friends about his wonderful cure, he often used the following words—'Now I feel like a bird, and can sleep like a child.' His temperate life generally helped him to a quick recovery. Perhaps the good results from your book were never expressed in the short and original way he did. His name is E. Sanderson Smith, Sumpter, Oregon. No relation of me.

For myself, I generally omit breakfasts, and eat but little of foods largely carboniferous, and derive much good thereby.

As so many on this side advocate free drinking of pure water, whether thirsty or not, I have been drinking three or four pints of water daily, but not during meals, though I seldom or never was really thirsty at the times.

I see that Dr. Dewey, in his late little pamphlet, takes very decided stands on that point, advising that no water be taken unless actually thirsty, and thirst should be confined to the three sources he mentions.

Do you wish to express an opinion on the question of drinking water?

Also, Dr. Dewey rather goes against special exercises. However, from experience, I am strongly inclined to take the view you do as to exercise.

Not many of us will agree with him on the subject of bathing the body, though his plan of omitting the morning meal, and his clear exposition of the harm and danger of eating to excess in any event, has done good to hundreds, and I suppose thousands, and has clearly prolonged very many lives, as, indeed, no doubt your work has done.

With best wishes for your happiness and continued success,

I remain,

Very truly yours,

S. M. SMITH."

As I say, I do not know how the narration in this letter will strike the reader; but I hope at least he will not grudge me the satisfaction I have felt in reading it. As to the points about water-drinking which Mr. Smith raises, I have already considered them. I think about two and a half to three pints of fluid taken daily is the gauge of moderation for the average townsman; and while I agree with Erasistratus (B.C. 300), that exercises are not necessary to health and life, believing that the movements supplied by the ordinary business of life are physiologically sufficient for that purpose, I think that these statements are true only for that small minority of persons who do not take too much food. For the majority, therefore, I still recommend the use of moderate exercises, especially for 10, 15, or 20 minutes in the act of dressing in the morning. They help to keep the body supple, young and active, and they render us more active and fit for the prosecution of the work of the day.

In the following case, chronic bronchitis, lasting for many years, led on to a severe attack of pneumonia, to which the patient speedily succumbed; but the causes of the bronchitis and of the pneumonia plainly were the same as in the cases just related. Some years ago I saw in consultation a stout man, 54 years of age. The patient had suffered for years from what his wife called asthma and bronchitis, the symptoms being a more or less constant cough, with whistling sounds to be heard in the breathing, and increase of the cough when

he hurried or undertook exertion. The man was a teetotaller (how often, as I have said before, are effects, which are really due to wrong eating, attributed to alcohol. They could not at least be so attributed in the case of a life-long teetotaller). As I say, when the family doctor and myself saw the man together, he had most severe and extensive pneumonia, and the doctor had justly given an almost hopeless prognosis, too speedily verified, in fact. Now what had this man's diet been for many years? I asked the question of his wife, and she told me as follows:—He was in the habit af taking tea and bread and butter for breakfast at 8 a.m. At dinner (12-30) he would have meat, Yorkshire pudding, bread, and rice pudding. He had abstained from potatoes for some time, because he thought them watery, and that they tended to puff or blow him up, and make him fatter than he was. (Note here how the man made the mistake of thinking that it was potatoes, rather than too much food, taken too often, that made him ill. He stopped potatoes, but continued to take bread four times a day. In point of fact, it is not generally this, that or the other article of diet that makes us ill, but it is too much food of any kind taken at too short intervals of time. We saw the same principle in action when dealing with cancer, and the reader will see that the causes of bronchitis and of cancer—as, indeed, of most, if not all other illnesses—are the same.) Tea at 6 p.m., of bread and butter and tea. Supper, 9 p.m., oatmeal porridge. The plain

causes of the chronic bronchitis and of the pneumonia which supervened on it, were the four meals eaten every day, bread being taken at every one of them (for the oatmeal at supper has much the same composition as wheatmeal), the Yorkshire pudding, and the rice, which formed practically the whole of the diet. Bread twice a day is enough for most townsmen, if they wish to keep well; and I have no doubt that three meals a day would have been much better for this man than four; while, if he had taken two meals daily, the probability is great that he would have been living now, without the help of doctors. It was not his teetotalism which killed him, not at all; but on the other hand it failed to save him, as it has in my experience failed to save a large number of other persons who persistently have ingested into their bodies more food than they could assimilate, or oftener than was proper.

So much for the dietetic management of those who suffer from bronchitis, asthma, and broncho-pneumonia. As to pneumonia itself now causing a calamitous mortality among the people, I have no doubt at all that it is due to the same causes as the affections just named. It is not, however, quite so easy to prove this, because pneumonia seems to attack people suddenly, when they are in the midst of robust health. It has, in fact, seemed true often that pneumonia is an affection of the strong (not always, of course, for delicate persons often suffer from it also, and it often enough ends life after a

succession of illnesses of other kinds). In many instances, however, persons attacked by pneumonia have had no previous illness at all for years. I remember very well a gentleman who told me he did not know he had a stomach, or a liver, or a heart, or lungs, or any organ, because none of them troubled him. Being so strongly made, he lived freely, and indulged in the pleasures of the table. I cannot doubt that this was the reason why he was struck down as by a bolt from the blue by an attack of pneumonia, to which he succumbed on the fifth day, his sorrowing family being deprived of his help just when they needed it most, and he himself of life at about fifty years of age. Recollections of this kind crowd upon the medical man as time goes on, and seem to emphasise his determination to draw attention to the effects of free living—effects which, when they have been persisted in for some time, are often irremediable, but which at an earlier stage could so easily have been prevented. Had he taken two reasonable meals a day, I believe that man would have been living now, and I believe he was strong enough to have resisted it, even had he taken at each of them rather more than he required. Of course I do not recommend this; but still many of us have so great a resistance that even when we over-tax our powers considerably (of course it must not be greatly overdone) we still live long and have good health,

Other recollections fill one's memory, and compel relation in order that, if possible, men and

women may be warned in time of the calamities which, although we attribute them to fate or other inexplicable "dealings of an inscrutable Providence," are most commonly brought upon us by ourselves. A heavy man, about forty-seven years of age, in the very prime and vigour of life, consulted me on account of a liability to "take cold" frequently without sufficient cause. I pointed out to him the causes of this, viz., that he was ingesting into his body more food than he required to maintain him in health and strength, punctuating my opinion by drawing attention to the facts that he had indigestion, and that he was too heavy for his age and size. He did not like the advice I gave him, and after coming back once or twice, ceased to return. In six months I saw his death announced from acute pneumonia. I quite believe that when the last illness overtook him, it was too late to save him. I do not suppose any other doctors could have done any better for him than those who were called in. But I am equally certain in my mind that if, six months before his death, when he came to see me, he had begun and had afterwards continued to follow the advice I gave him, he would, after suffering at first, perhaps, from an aggravation of his "colds," &c., have begun to recover from them, that he would have gradually thrown them off, and that the calamitous illness which made his wife a widow and his children fatherless would not have occurred, or if it did, that it would not have terminated fatally.

The medical man, like any other scientific man, does not make the law; if he did, he might be tempted perhaps from a weak sentimentality to make it less stringent, though I do not for a moment think that that would be a good thing; but the scientific man's duty is to declare the law, the responsibility then resting with other people whether they will obey it or not, or whether they will recognise it or not. Whatever they do, however, and whether they ignorantly or knowingly disobey it or obey it, the effects are inevitable, and no amount of protest or of frantic appeal to be saved from the consequences of disobedience are of any avail whatever.

As to rheumatism and gout, very much evidence has accumulated in my experience to justify me in saying that I am satisfied that both of them are due to the persistent ingestion of food into the body over and above its requirements. I have already remarked upon the name rheumatism, and given the reasons for the suggestion that we should displace it in favour of the term or name initis, in order to get rid of the obscurity attaching to it. I wish only to add here a word or two about the derivation of the terms rheumatism and gout, so as to complete what has to be said about them. Rheumatism comes from the Greek ῥεῦμα = a stream, and the name was applied to the pain which succeeds on the stopping (generally the sudden stopping) of a watery discharge or stream of watery matter from *e.g.*, the head. To this day the term rheum is used by some elderly people of a watery discharge from

the eyes. As the watery discharge comes from the lymph and blood, and as the blood gets its materials from the food, the name is not a bad one; but my reason for wanting to be rid of it is because it has long ceased to have that meaning, and because it is used of so large a variety of conditions, that those who use it attach no definite meaning to it. Rheumatism *is* no doubt primarily congestion of connective tissue, or inflammation of it, which is a further stage of congestion. The next stage is effusion, effusion either of a solid or of a fluid exudation, which exudation, however, is the succeeding stage of all inflammations. Wherever connective tissue is found in the body, it may be congested or inflamed; and as connective tissue forms the coverings of muscles, nerves, and bones, and as it forms also the ligaments about joints, as also much of the tissue under the skin, rheumatism may arise whenever connective tissue in any of these very widely different parts of the body is inflamed. Tendons also are made of connective tissue, and hence *tenonitis*, or inflammation of tendons is a form of rheumatism. Other forms are neuralgia, muscular rheumatism, periostitis, shewing themselves in general aching, or varied symptoms, according as the connective tissue of the nerves, of the muscles, or of the bones is affected; and of course the ligaments of joints may be inflamed when we get the joint inflammation or arthritis, which some authorities say is the only real rheumatism. I do not agree with this view at all,

because, wherever connective tissue is, it may be inflamed, and inflammation of connective tissue is rheumatism. Connective tissue is found also to a considerable extent about the brain (as well as forming the sheaths, &c., of nerves), and hence many phases of mind-disturbance are associated with its congestion, the flightiness, *e.g.*, uncertainty, irritability, unreasonableness, depression, excitement, and so on, with various phases of intellectual and moral perversion and perversity, which, as I have already dealt with them in Chapter VIII., I need not here recapitulate. The feeling of "sinking" which the women describe who suffer in this way, and the far too frequently repeated small meals with which they attempt to relieve it, reminds one forcibly of the craving for alcohol described by the alcoholic, which he in turn attempts to relieve by small and frequently repeated nips or drams. In both sets of cases the immediate effects of indulgence in small and frequently repeated meals and in small quantities of alcohol are to relieve the sinking and the craving, and in both the remote effects are most disastrous. In both sets of conditions the best method of treatment is abstinence from alcoholic drinks on the one hand, and from food on the other, at least for a considerable length of time; but the latter advice is as yet very seldom given, and still seldomer acted upon. However, as I have dealt with the matter already, I will not dwell upon it now. It has obviously come on again for discussion from our consideration of the term

and disease rheumatism, whose definition I take to be congestion and inflammation of connective tissue, and which I therefore consider to be synonymous with *initis*. (Syndesmitis, from συνδεσμεῖν = to bind, would really be the best name, but unfortunately this name is not available, because it has already been appropriated to inflammation of the ligaments of joints, which is, of course, one form of connective tissue. One may add a new name to Science, but to attempt to divert a name already in use to a meaning different from that to which it has already been appropriated, even when the suggested change is an unquestionable improvement, only leads to confusion.) The term gout, again, is connected with the Latin *gutta* = a drop, and both rheumatism and gout were therefore considered by the older writers to be defluxions from the blood. This, indeed, they are, or at least they are either fluid defluxions from it or solid exudations from it, so that the older ideas regarding these affections were quite correct, and if we remember that the blood gets its materials from the food, we have a perfectly correct idea of both. Fundamentally, the ancient idea regarding both rheumatism and gout that they were caused by the deposition from the blood of materials present in it in excess was quite sound. Modern investigations have shewn that uric acid or some of its compounds is a common form that this exudation takes, but they have not altered the principle underlying the ancient notion. In rheumatism many joints are affected, the knees,

ankles, elbows, shoulders, jaws, and many smaller joints in the fingers and toes, the toe nails (and even the finger nails sometimes) being often distorted and overgrown. (How, it may here be asked, should this be, unless the blood, containing more material than it requires, converts its excess into overgrowth?) Gout, on the other hand, is used of the disease in which one joint, that at the ball of the great toe, is specially affected. The important thing, however, to remember is that the blood contains nothing which it has not received, and that the source *par excellence,* the source compared with which all others are comparatively unimportant, from which the blood obtains the materials contained in it, is the food. In fact we eat gout and we eat rheumatism in the sense in which Sir Thomas Browne said that "we eat our bodies on our trenchers," and "devour ourselves," viz., we manufacture our diseases, and rheumatism and gout among them, out of an excess of food.

To shew that restriction of the diet often cures rheumatism, take the following cases, which could be greatly multiplied. A lady, aged 30, had suffered very severely from chronic rheumatoid arthritis for seven years. She had stiffening, swelling and distortion of her fingers and wrists, much pain and stiffness in the back of the head and neck (an affection literally of the *ἰνίον,* or strong tissue at the nape of the neck), and she had also a large fluid effusion into one knee joint. For this I recommended her to eat once a day (to be

monositeous), and to take a very small meal then. She did so for about three years, and under this treatment she improved greatly in health, the rheumatoid arthritis ceased to spread, the extensive effusion into the right knee-joint was absorbed and disappeared, and the pain and stiffness in the back of the neck were greatly diminished. The diet was often weighed, and consisted of seven, eight or nine ounces of mixed food daily, a much less quantity than is generally taken or than is usually required. More strange is it to add that on this diet she gained four pounds in weight in about three years.

In another case a man had suffered intermittently for years from recurring attacks of rheumatism, which affected his muscles rather than his joints; that is, he would find the great muscles of the back, or of the thigh, or of the leg, painful and sore as if from over-exertion, although without adequate cause of that kind; or if he had to hurry to catch a train, for instance, he would find the muscles of the calf or of the front of the leg stiff and sore for perhaps a week afterwards. Sometimes he would find himself suffering from a "crick" in the neck, so that he could not move his head freely from side to side, and if he wanted to look round would have to move his whole body in the slow deliberate way adopted by those persons who suffer from disease of the spinal column. In addition, he suffered from stiffness and aching about the fore-arms and about the elbow-joints, and sometimes about the shoulder-joints. One of the

ways in which such conditions resulted was a feeling of great fatigue on making small exertion. For instance, on making such small exertion as is involved in carving the meat at dinner for the family, the aching of the arms would be so considerable, and the feeling of fatigue so great, that he would have to drop the knife and fork for a few moments until rest enabled him to go on again. Obviously, so slight an exertion could not possibly be the adequate cause of such fatigue. And, generally speaking, the alleged cause obviously never was more than the occasion of the annoying feeling of fatigue and of being "overdone." These symptoms would come on somewhat suddenly and painfully, and after lasting for a longer or a shorter time, would pass off again with as little apparent cause for the relief as there seemed to be for the attacks. From the age of 30 up to about 50 years these attacks annoyed him, though without ever being severe enough to lay him up, or not for more than a day or so at a time ; but the attacks were getting worse. A point in the case was that when about 50 years of age his mother had had an attack of chronic rheumatism, as it was called, which confined her to bed for several months, although it had not hindered her from afterwards attaining advanced life. Here seemed then to be a case of hereditary rheumatism in the conventional sense. The reader, will, however, now be prepared to view the rheumatism in mother and son as brought on by similar causes acting on similar organisms in two

generations rather than as cases of inherited disease, since the man did not begin to suffer until he was grown up; and also from the sequel of the case, which was this, that when under medical advice he reduced his meals from three, four or five a day to two, and subsequently to one, he was able slowly to get rid of all his troubles, to walk and move and run freely, and to suffer only at very greatly lengthened intervals from his recurring muscular stiffnesses and pains, which, when they did come, were less and less severe, and finally wore off. The mother had had to slowly learn the same lesson also, and although she never did recover to the same extent as her son, still she was able to live in comparative comfort to over 80 years of age. But so far from the disease being hereditary, the evidence was simply to the effect that both mother and son took more food and more meals than were good for them, and having similar constitutions or organisations (for organisation is inherited, though disease very rarely is), suffered in similar ways; also, when the causes were diminished, that is, when the excess of food was diminished, both mother and son recovered somewhat, the former a little, and the latter practically completely. As to the second part of the proof that a recurrence to the action of the same causes re-induces the same conditions of body, I have no doubt at all that if either the lady first mentioned, who suffered from rheumatoid arthritis, or the man, should return to their former way of living, they would find their

disease in both cases aggravated. But I do not recommend either of them to have recourse to such an experiment.

The same considerations enable us to understand many or even most other diseases; and management founded upon our proper understanding of them enables us to effectually deal with them. I have already (p. 318) mentioned anæmia, which ought to be named triphthæmia or catatribæmia, as has been said. It is not want of blood (anæmia) from which anæmic patients suffer, but it is loading of their blood with an excess of material or of waste matter, which is the real malady. As the chief source of this is excess of food, the chief means of relief and cure is and must be restriction of the diet; and hence the young woman who had seen eight doctors without relief was greatly benefited, and in a few months cured by the diminution of her food, her blood being allowed to flow better to her too solid tissues, as the obstruction to its passage was slowly removed. I need not repeat what has already been said, nor detail what I hope the imagination of readers will easily fill in each for himself. The young anæmic woman was thin, pale, bloodless-looking, and wasted because her blood and tissues were checked or obstipated by unused stuff which had entered her blood from the digestion, and therefore relief was to be expected, and in fact was obtained only by restriction, the opposite method in the hands of eight different doctors having failed to relieve her, or to do

anything but aggravate her misery. Many or even most other affections which thin and waste and lower the physique of patients must be treated in the same way. Neuralgia, megrim or migraine, and sick headaches, for example, are other examples of similarly caused diseases, which must, therefore, be similarly treated. These diseases are recurrent or periodic, as is well known. Patients get an attack of neuralgia or of megrim, or of sick headache (a bilious attack, as it is often called), and then they recover and go on for a while till another attack occurs; and if one has not in one's mind the organic law that constant causes acting on the organism shew themselves not in constant but in periodic or intermittent attacks, one is very apt to be deceived as to the causation of these attacks, and to imagine, as in fact patients do, that it was that draft or that fatigue, or that crowded room with its bad air, or that exposure to wet or cold, which caused the attack, and not the wrong food habits of the sufferer which were the real cause. We confound the occasion with the cause or mistake the exciting for the predisposing cause. The proper translation or application of the law, in fact, is this; Do we find ourselves suffering from a recurring affection of any kind, then we ought to search for a constant cause; and in the large majority of cases that cause will be found to be wrong food habits. A better statement of the law would be (since organic actions are not so much constant as intermittent) that causes acting at short intervals of

time shew themselves in effects which appear at longer intervals of time. Food, for instance, taken wrongly four or five times a day at intervals of three or four hours, day after day and week after week, causes effects which appear at longer intervals of time, headaches, *e.g.* or neuralgic attacks, or "bilious attacks," occurring at intervals of weeks or even months apart. And the application of the law is this: Do we find ourselves suffering from such attacks, then we should alter our habits, and particularly our food habits—*i.e.*, we ought to lengthen the intervals between our meals. This is the meaning of the statement of the ancient writers on medicine, of Galen, for example, when he said that recurring affections are to be treated in the intervals rather than during the attacks. As epitomised by Andreas Lacuna Secobiensis, he says *administramus remedia, quum morbus non adest sed futurus timetur* (we administer our remedies not when the attack of the disease is on, but when we have reason to believe that it is impending). In his own interesting words he says:—*εἰδὲ ἀκμῆς παρούσης νοσημάτων οὐκ εἰσὶ καιροὶ τῶνβοηθημάτων, καὶ μὴ ὀντων νοσημάτων εἰσὶ καιροὶ βοηθημάτων, φανερὸν, ὅτι οὐκ ἐπινοίᾳ μόνον ἀλλὰ καὶ ὑποστάσιν διαφέρουσιν.* (For it is evident that the times of the occurrence of the paroxysms of (recurring) diseases are not the proper occasions for the administration of remedies, but that, on the other hand, the times for favourably affecting (recurrent) diseases are the intervals when they are not present,

since opportunities for treatment differ not only in our view of disease, but in its own nature.) We can, in fact, do little except to attempt to palliate or relieve an attack of neuralgia or sick headache or "biliousness," or of recurring bronchitis or broncho-pneumonia when it is on, but we can do much to prevent subsequent attacks, to lengthen the intervals between attacks, and to render the attacks less severe, when or if they do occur. But recurring attacks tend to become, and are on the way to becoming, chronic or long continued, and the chief way in which we ought to handle chronic illness also is to alter the habits, and particularly the food habits. For even chronic illnesses, that is, illnesses which are always present, are not always *equally* present, or are not present always in equal degree or extent. Like the recurring affections from which they are developed, they have their periods of exacerbation and of recession, they are better and worse. And they often go on to or pass into other affections, or what seem to be other forms of disease. Recurring sick headaches or bilious attacks frequently, for example, lead on to attacks of chronic rheumatism or to the formation of some bony or other tumour or growth, which could have been prevented had the patient been advised earlier how to deal with the sick headaches in the intervals between the attacks. A woman, for instance, whom I saw for recurring sick headaches many years ago, and whom I was able, by the administration of medicine, greatly to relieve from

the severity and frequency of her attacks, sent for me some years afterwards on account of the onset of inflammation, pain, swelling, and stiffness of the joints of her fingers, of the nature of rheumatoid arthritis; and some years after that time I saw her again for an incurable bony tumour of her pelvic bones, a form of cancer no doubt. Twenty-five years ago, when I first saw this woman, I was not so profoundly impressed as I am now by the power of food to cause health and to cause illness, nor did I then know how much more helpful I might have been to that patient than in fact I was. If I were to see her now for the first time, I should strongly impress on her the advisability of eating twice a day, and twice only (she is a teetotaller, so that here again the cause is not alcoholic drink) up to say fifty years of age, and once daily after that age; so that the cause of the evils, which I did not then foresee, but which I now know to have been wrong living, might not continue to act, and so that she might, escaping both her rheumatoid arthritis and her incurable pelvic disease, have settled down into a serene, painless, and placid old age. Perhaps she might not have taken the advice; but, however this might have been, I at least should have been able to feel that I had warned her beforehand of what was impending, and so have afforded her the opportunity of escaping, if she wished, from the evils before her. So much in the way of evidence to shew that gouty and rheumatic affections depend upon wrong food habits more than upon any other

single cause; that their cure and amelioration, therefore, depend rather upon restriction of the diet than upon any other single measure; and that recurrence to former food habits is the most potent influence in re-inducing the former disease-conditions. Of course other causes may be contributory to the induction of rheumatism or gout. Damp, wet subsoil, for instance, and bad drainage may be subsidiary causes, and therefore some improvement and amelioration may be obtainable by removal to a drier soil (from clay to sand, for instance) and to better and drier climatic conditions; but, on the other hand, much improvement and even cure are often obtainable without having recourse to these costly adjuvants, beyond the reach of the means of many sufferers, by the simple device of restricting the diet.

As to the cure of recurring ailments, like sick-headaches, bilious attacks, and the like, and the relation of these affections to ailments so wholly different apparently, as, for instance, tumour-growths, take the following case. A woman aged thirty had a large ovarian tumour which it was necessary to remove by operation. She also suffered from dyspepsia, anæmia, and recurring sick-headaches, and "bilious attacks," which would confine her to bed helplessly for a day or two at a time at weekly intervals or so. After I had removed the tumour, and had so obtained her increased confidence and that of her husband and family, I advised her to eat once a day, and once a day only,

in order to prevent the formation of some other tumour or morbid growth in the body, as also to obviate the recurrence of her indigestion, her anæmia, her sick-headaches, and her bilious attacks. The effect of this advice was immediate in the great amelioration of all her symptoms; and now at the close of about a year after the institution of these changes, or I ought to say of this one small change in her mode of living, she has no dyspepsia, she is relieved of her constipation, and has no more than a suggestion from time to time of her former sick-headaches and bilious attacks, and has never been laid up for a day by the enemies which used so to annoy and worry her. That continuance in this way of living, besides ridding her of present ailments, will be the best means also of preventing the onset of other diseases, like the chronic rheumatism, the rheumatoid arthritis, or the bony growth suffered from by the other woman referred to, as also that it will probably afford the best protection against the formation of some other malignant growth in the body, and against the occurrence in the stump of the removed ovary, of some cancerous growth, I have and can have no doubt. And if there is any self-restraint required, or suffering endured by voluntary submission to such restriction, it may reasonably be contended that these inconveniences are much less and much more easily borne than the compulsory restraint, and confinement, and suffering, and annoyance inflicted by nature as the penalty for breach of her

laws. The nexus between cause and effect does not seem to me to be at all difficult to see ; but whether we see it or whether we do not, the penalty comes inevitably for any breach of nature's laws. And conversely, when we find ourselves suffering from any recurring ailment, we ought to inquire whether we are not in some simple and plain way breaking some natural law, and whether by the means of treatment we are adopting for it, by the use, *e.g*,, of an aperient, or of hypodermic injection of morphia, or of antipyrin, phenacetin, phenalgin, or other drug, we are not simply palliating and masking symptoms, which would disappear of themselves if our ideas of treatment were to go back to the digestive causes of our sufferings. In very few cases, indeed, would such a line of treatment fail of affording relief. When it did we might perhaps begin to institute inquiries further back still, and see if some hereditary predisposition were not at the foundation of our suffering—which, if it were so, would raise considerations which I will not pursue further at present.

Another case in which recurring sick-headaches were cured by changes in the diet recently came under my notice, and I mention it here because it illustrates how the law can be put in action without the intervention of the medical expert at all, just as any other law of nature can be in any other department of nature when the law is known. A young lady, 22 years of age, recently told me this interesting story. She suffered, she said, from the

age of ten or twelve years from recurring sick-headaches, sometimes accompanied by severe vomiting, which would compel her to betake herself helplessly to bed for a day or perhaps two days at a time, during which time she could take no food at all. These attacks continued up to within two years of the time when I met her, that is, up to 20 years of age. About that time, partly through reading one of Dr. Dewey's books, and partly through other reasons, she adopted, and that in spite of the ridicule of some of her young friends, a dissiteous regime, in place of the trissiteous or tetrasiteous course which she had formerly followed. Almost from the very first, she said, she had experienced benefit, and in a few months she was quite cured, by taking no more than two daily meals, of headaches which had annoyed her for not less than eight or ten years. When I saw her she was nursing her little brother, and, as he was very ill indeed, her duties compelled very close waiting upon him. As she could not leave his room to take any exercise in the open air, she thought she ought to become for the time being monositeous, that is, that she ought to eat only once a day; and at the time of which I speak she had been doing this for a fortnight, a course in which I encouraged her to persevere as long as the circumstances lasted which led her to adopt the plan. Of course the original suggestion of dissiteism, or the later one of monositeism, was not mine at all. She had heard of the law that sick-headaches were due to too many meals;

she applied the law and got cured. The suggestion subsequently of monositeism to meet her temporary deprivation of exercise was also of her own making, on the very reasonable ground that as there was so much less expenditure of physical and kinetic energy, there was the less reason to ingest so much potential energy into the body, as before, in the form of food; and her rosy, plump, and healthy appearance appeared to me to completely justify her application of the law.

The same principles underlie the causation of all recurring affections of the animal economy. What the tissue or organ which is affected may be is an accident. We have seen how the law applies to affections of the bronchial mucous membrane, and of the lung tissue, to rheumatic affections, and in cases of sick-headaches. But affections of any and every part come under the same law. For instance, here is a case of skin affection which was successfully treated by dietetic management. A young woman, who had suffered for many years from recurring attacks of erysipelas of the face, and who for many months before I saw her had been prevented by her illness from following her occupation as a weaver, and whose face was distorted almost beyond recognition by repeated attacks of her malady, was informed that her diet, consisting largely of bread, potatoes, puddings, and sugar, was the chief predisposing cause of her recurring illnesses. (The essence of the cause was too much food; the accident was too much starch and sugar.) She was

advised, therefore, to eat bread only once a day, and then not more than four ounces, and to have for dinner, if she could get it, three or four ounces of any meat, with some lettuce or other green vegetable. She was also advised to diminish greatly her puddings and sugar. She did so, and very soon the attacks of erysipelas began to come less frequently and with diminishing severity, and in the course of a few months she became very much better. As she had improved so much she after a time thought that she need not be so particular about her diet, but might do as other persons of her acquaintance did. (It is almost always a puzzle to patients when they are told that so-and-so is the cause of their illness, to answer the question, why is it, then, that the same causes do not make their friends and acquaintances ill in the same way as themselves. "If," they say, "I am made ill by bread and puddings, why are not A.B. and C.D., whose habits I know very well, and who take even more of these foods than I do? Why are they not made ill in the same way?" The answer that different persons have different resistances to these things, and to the physiological labour required for their digestion and assimilation, does not seem to satisfy them. It is, perhaps, not a very satisfactory explanation, although it is all the explanation which we can give.) My patient, at least, thought she might do as her friends and acquaintances did, and after recovery took more of these foods than she had been advised to take. The *erysipelas faciei*

recrudesced, after which she made no more experiments, but, restricting herself to what she had been advised, remained well. The proof that the alleged cause of the erysipelas was in such a case the real cause requires no additional confirmation.

The successful treatment of erysipelas of the face by alteration of the diet raises another interesting question besides merely that of the treatment and successful prevention of a recurring inflammation. Erysipelas has been shewn to be associated with (the common view would be that it is *caused by*, but I have already frequently given reasons for dissenting from this interpretation of the facts) the growth of a micro-organism in the blood and tissues. This being so, erysipelas asserts its relationship to the fevers, whose course is associated with the growth of these micro-organisms, and not merely of an inflammation whose course is not known to be so associated. Whether the micro-organism is a coccus, as in this instance, according to Fehleisen, it is, or a bacillus, makes no more than an accidental and unimportant difference. On this view, if erysipelas is not a fever, it is at least a "specific inflammation." At any rate it is not a simple inflammation. But whether we view it as a fever or as a specific inflammation, the very interesting and very important fact stands out that the presence of the microbe does not render the disease any less amenable to dietetic treatment than if it had been a simple inflammation which was not

associated with the growth of micro-organisms at all. However the coccus entered the system, it thrives in tissues that are over-fed, particularly as it happens with bread and starchy stuffs, although the kind of food may be (?) an accident. And this being so, whenever the over-manuring of the soil is reduced to what is fitting and proper, the disease dies down, or rather, and as I much prefer to say, the body (that is the soil) returns to a condition which renders it unsuitable for the continued growth of the coccus, the coccus dies down and the patient recovers. The whole question of the management of fevers is raised by this simple issue, and it is on evidence of this kind, and on this kind of reasoning among others, that my opinion is founded that in the management of the fevers more attention should be devoted to dietetic conditions, and that dietetic conditions should be considered by the medical adviser more than questions merely as to air-supply and overcrowding and infection. Until we deal with food-supply, I do not think we ever shall effectually deal with fevers (or with consumption). I have just to add the single word that erysipelas is an affection so infectious when once it has started, that surgeons have the most wholesome horror of allowing it to be near wounds, since they know that its presence is very apt to set up blood-poisoning in their patients. Even in this case, however, something, nay, very much, will depend on how the patients suffering from wounds have been fed—although, of course, I freely admit the duty of the

surgeon to keep his patient clear from all sources of infection as far as he can. These facts, however, seem to me to be very instructive as to the causes, the origin, the spread and the treatment of specific inflammations, and their relations to fevers.

In erysipelas the skin is red, even very red. Redness, in fact, and erysipelas mean much the same thing. Pain, redness, heat, and swelling are the cardinal symptoms and signs of inflammation, and erysipelas is inflammation of the skin (and connective tissue beneath it), and as it is accompanied by the growth of a micro-organism, it is a "specific inflammation" of the skin. Now the redness of the skin being attributed to an excess of food, and being treated by restriction of the diet, it may seem somewhat strange to attribute the extreme pallour which we find in anæmia (*triphthæmia* or *catatribæmia*) to the same cause, viz., an excess of food, and generally an excess of starch and sugar. I hope, however, that the reader is now accustomed to the idea that defect and excess of function are often due to the same cause. I should, however, like to add a word or two here to shew how excess of food so frequently acts in causing the pallour of anæmia. In anæmia the muscular fibres of the vessels, and particularly their transverse or circular fibres, are first hypertrophied, as it is called, or overgrown. When they are overgrown, they tend to go into contraction, and the effect of this being to diminish the calibre of the vessels, less blood gets into it, and pallour of the skin, if the vessels in

question are skin-vessels, results. But the muscular fibres of the vessels become hypertrophied because the blood contains in it more material than it requires for the effective nourishment of the tissues. The pallour or anæmia is therefore a direct effect of the over-nutrition of the fibres which automatically act so as by cutting off the blood-supply to diminish their over-growth. But inasmuch as relaxation always follows contraction, and is further apt to be proportional to it, the greater the contraction the greater the subsequent dilatation, the same causes which primarily produce the pallour of anæmia secondarily produce the over-redness of erysipelas; and by observation and interpretation of these processes we see how the same cause, viz. an excess of nutritive material in the blood, produces both of these opposite states. The same reasoning enables us to understand, when the necessary name changes are made, how over-nutrition by causing hypertrophy of the transverse muscular fibres of the bowel may induce constipation, and then when dilatation follows, may induce diarrhœa. And the practical conclusion, of supreme importance to right treatment, follows, that in order to cure the pallour of anæmia and also the too great redness of erysipelas, we must restrict the diet; while it also follows that in order to cure constipation and its opposite, diarrhœa, we must restrict the diet likewise. These opposite conditions are not usually found both to be present in the same person at near periods of time—they cannot, of course, both be present in the same

person at the same time—hence our rule or canon of treatment generally takes the form of compelling us to treat pallour in A by the same means by which we treat over-redness or erysipelas in B, viz., by restriction of the diet. And the same considerations apply, *mutatis mutandis*, to the treatment of constipation and diarrhœa. Nevertheless, as even in the same person anæmia may be present at one stage of illness and erysipelas at another, and the same being true of constipation and diarrhœa, it follows, or may follow, that we may have to treat even in the same person, at somewhat longish intervals of time, opposite states in the same way, viz., by restriction of the diet.

Other skin affections which are due to over nutrition, and particularly to over ingestion into the economy of starch and sugar (not necessarily, be it always remembered, unwholesome food), are eczæma and pruritus, or excessive itchiness of the skin. A very annoying form of this latter affection is frequently experienced at that part of the skin which lies close to the orifice of the bowel and is called *pruritus ani.* In this affection patients scratch themselves till they bleed, doing it often even in their sleep, and waking themselves up in great misery. Many remedies have been suggested for its relief, and some of them are useful; but the great remedy is restriction of the diet, and usually restriction of the carbonaceous elements in the diet. I detailed a case in the first edition of this essay, and I repeat it here. A stout woman, aged 41, who

had suffered from this complaint for a long time, presented herself as an out-patient at the Bradford Infirmary in August, 1896. She had suffered from time to time, for no less than seventeen years from the intolerable itching which characterises this affection, and had sought advice and assistance from several quarters, though without obtaining relief. She used to suffer from diarrhœa (due, no doubt, to fermentation of carbonaceous food) but this had subsided of itself before she came under my care. She had been subject also to attacks of "influenza colds" from time to time, that is, had attacks of sneezing with cough, but had never been laid up with bronchitis proper. When I saw her she had very disturbed nights, on account of the itching, which made her scratch herself in her sleep, sometimes till blood was drawn. By the simple advice of recommending her to take bread only once a day, and greatly diminishing other forms of carbonaceous foods, I was able entirely to relieve this long-standing ailment, so that in about five weeks from the time of her coming under treatment she was able to sleep right through the night. In another fortnight all the eczæmatous spots (I suppose *pruritus ani* really is eczæma ?) had healed.

There now occurred a rather remarkable incident. For some time I lost sight of the patient, who absented herself from the out-patient room from the end of December, 1896, till February, 1897, when she was nearly as bad as ever again. What was the reason of this relapse ? She told me

she had had a miscarriage, and had been obliged to consult a medical man. He had put her on the conventional treatment usually adopted in that condition, ordering her to live on gruel, bread and milk, rice and milk, &c. The increase of starch taken brought back the itching, and she was obliged to make her way back to the Infirmary as soon as possible to obtain relief again, in which she was not disappointed. Here, then, is a case in which a medical man—first, alleges that the main cause of an ailment lasting for seventeen years is wrong eating (and particularly that it is eating too much bread and carbonaceous food); second, in which he therefore recommends that bread be taken only once a day. Thirdly, the patient recovers, or *practically* recovers (she was not *perfectly* well) on taking his advice. Fourthly, she is compelled to put herself under advice for another ailment; and for the management of that ailment is recommended to live on the kind of food which was alleged to be the main predisposing cause of her first ailment. Fifthly, the first ailment returns in accordance with the original statement. Sixthly, the ailment again disappears when she again follows the original advice. I do not know what more clinical evidence could be demanded by the most sceptical in the way of a demonstration as to what the causes of the ailment were. The logical methods of agreement and difference combine to demonstrate that the main predisposing cause of *pruritus ani* is wrong eating, and in particular that it is the consumption

in excess of starchy food. She suffers from the ailment when she takes the food, and she ceases to suffer when she ceases to take it; or rather (since it is not abstinence but moderation which is inculcated by nature) when she sufficiently diminishes it. Now this conclusion does not apply to this case only, but to practically all cases where the experiment is made. The conclusion that the alleged cause is really the main cause has become firmly fixed in my mind, and has indeed the force of a law of nature or of human organisation. And, of course, anyone can for himself put the law into action when appropriate circumstances arise.

There is, however, a further line of evidence possible, in order to prove this conclusion up to the hilt. If, as I suggest, starch and sugar is the cause of *pruritus ani*, how does starch and sugar cause it? Perhaps by carrying into the stomach and thence into the intestines, and thence again into the blood, some spore or seed which grows by preference at the orifice of the bowel? Might it not be possible to demonstrate the existence of this spore first in the starch granules of the food, and second, in the blood? Such spores when in small numbers are no doubt destroyed by the digestive juices, but it is conceivable that, if ingested in too large numbers, that is, when too much of the food containing them is consumed, they may escape digestion, and find their way into the blood, and so be carried to their favourite site. Of course this is a mere suggestion, but it does not seem at all an impossible means by

which the disease may be conveyed, although there is no proof up to the present time that *pruritus ani* depends upon bacteriological growth at all.

Cases of this kind could be multiplied to weariness. Indeed some critics seem to complain of the nauseousness of the proof of the dependence of disease on wrong eating. But what can one do? If one says that such is the case, and that the taking of too much food is the chief predisposing cause of the large majority of the ailments to which humanity is subject, one is asked for proof. It is a perfectly reasonable request, and ought to be met. But when it is met, it is rather unfair that one should be twitted with the reproach that the proof is excessive and wearisome.

Sycosis. A very chronic and intractable form of skin disease is one which attacks the chin and cheeks and upper lip in men, and is often said to be induced by the use of dirty instruments at barbers' shops. It is called *acne mentagra* (acne of the chin), or *sycosis menti,* and consists of a number of pustular points, suppurating, itching, and unsightly, affecting the chin or upper lip or cheeks. It is of two forms, and is known to be due to the growth of a parasite, either the *Tinea Barbae* or the *Staphylococcus aureus et albus.* This is the authoritative statement, although I regret to be unable to accept it in this form from what I am about to add. I think it is a much truer statement to say that the disease is known to be associated with the growth and development of the *Tinea Barbae* or the

staphylococcus aureus et albus, than to say it is due to it. The treatment recommended is usually painful and long, and consists in epillating or removing the affected hairs with pincers, and in the free application of germicide ointments, &c., &c., and the treatment is admitted to be "unsatisfactory." I am sorry to add what I have to say, because it seems to conflict so much with ordinary views; but I find this one of the easiest affections to treat, and one which does not require any long time either, not longer than say eight or ten weeks, if the simple plan is had recourse to of restricting the diet. A young man, who suffered long and greatly from this affection of the skin and cheeks, affecting the hair follicles of beard and whiskers, was easily and painlessly cured by the simple device of stopping his breakfast, otherwise, by putting him on two meals a day. I thought the case so interesting that I asked the youth to report himself to a senior colleague of mine, a former hospital surgeon, who corroborated the diagnosis and concurred in admitting the efficacy of the treatment. The rationale is again quite simple. There is no difficulty about it. Too abundant eating does not (probably) cause or create the growth of the parasite which is associated with the disease (although the spores of it may be swallowed in food or drink, and, being ingested in great excess, may escape or elude the activity of the digestive juices). But, however the parasite got into the economy, there it is; and the question is whether

is it better (*a*) to attack it directly by attempting to extract it or weed it out, so to say; or (*b*) to kill it by making the soil in which it grows unsuitable to harbour it? As I believe the disease to consist not by any means so much in the presence of the parasite as in the state or condition of the soil in which it grows, that is to say, the state of the body and tissues, it follows that our efforts at treatment ought to take the line not of up-rooting the parasite, but in attempts to render the body unsuitable to harbour it. The line of treatment is determined by the analogy so often referred to now that an over-fed body is like an over-manured soil, and as the latter tends to grow noxious weeds, so the former tends to harbour the growth of parasites. Make the soil sound by proper drainage and proper manuring, and suitable crops will be produced; make the body healthy by proper feeding, and drain away impurities by restriction of the diet, so that any excess of pabulum shall be used by the body, oxidised and converted into the heat and motion of life; and then the parasite, finding the conditions no longer suitable for it, will die, acne spots will heal, and diseases like *sycosis menti* will disappear. The reader will not fail to see that the principles underlying the management of this affection are exactly the same as those already discussed when we were considering the nature of fever and of tuberculosis. Well might Asclepiades of Prusa, the medical adviser of Cicero say, Μὴ ὁ πυρετὸς, ἀλλὰ τὸ πυρετεῖν, *non febris sed*

febricitare—" the important things for doctor and patient to consider and weigh are not whether the patient has scarlet fever, or diphtheria, or influenza, or small-pox, but the fact that he has a fever at all." And he might have added, as he would no doubt have added had he lived to-day; the important thing to be considered by him who has *sycosis menti*, or by the doctor who would treat him skilfully, so as to prevent, for instance, the disfiguring keloid which is so apt to mark the face of those in whom the disease has run an unchecked course, is *not* that *sycosis* is caused by a parasite, but that a parasite of any kind should be thriving in the body at all. And even if a man thinks he got or caught his *sycosis* at the barber's, it will be well for him to inquire how it happened that so small a cause induced in him so loathsome an affection; as also to inquire how he may most readily in future make himself immune to such influences.

I ought to add, before leaving the subject of the skin and skin affections, that the skin behaves as other tissues and parts of the body do, and that the circulation in it is disturbed in the three directions of excess of circulation, of defect of circulation, and of irregularity of circulation, by excess of irritation, the commonest form of which is excess of food. The reader will therefore be prepared to draw for himself the conclusion that if the skin is too moist on the one hand, or too dry on the other, if it is too hot or too cold, or if too much moisture alternates too rapidly with too much

dryness, or too much hotness with too much coldness, we should in all cases look for a constant irritation ; and nine times out of ten, or at least three times out of four, we shall find that that constant or frequently acting irritation is too abundant or too frequent feeding. Even loss of hair, as shewing such congestion of skin as cuts off its own blood supply, so killing the hair follicles, has often to be treated by restriction of the diet. The occurrence of keloid or the white shiny state of skin left like the scar of a burn after sycosis has been cured by destructive inflammation, shews the condition of skin to which in some cases complete and permanent loss of hair is due.

As I have been referring to the skin and hair, perhaps this will be the best place in which to treat of ulceration. The term ulceration is applied to death of a portion of any surface of the body, and most usually to death of a portion of the outer surface of the body or skin, although, of course, it may refer to death of a portion of an inner surface, as the mucous membrane, lining the intestines or stomach or lungs. Wherever it is found, the same principles underlie both the mode of its occurrence, and, therefore, its proper treatment. Ulceration, like most (or all ?) other diseases, may be due to one of two opposite causes, starvation and plethora, or, as we have now learned to call them, direct and indirect starvation. The former form we very rarely see, although we often think we see it. But if an animal is literally starved to death, it suffers

from various forms of ulceration before it expires. It may suffer from ulceration of the skin, of the eye (sloughing of the cornea), or of some mucous surface. Of course, this form of ulceration must be treated by cautious increase of the diet, and if, for example, men had been exposed in a boat fifty days with ten days provisions on board; or if miners, being entombed by accident for a similar length of time, were found to be suffering from various forms of ulceration, it would be necessary to cautiously feed them with an increasing amount of nutriment in order to cure it. But how rarely do we see this. One of the commonest forms of ulceration is the callous ulcer, as it is called, very often appearing as a large ulcerated surface on the leg, surrounded by a raised thickened mass of connective tissue an inch or two inches broad, and an apparent three-eighths of an inch thick, and very often accompanied by thickening of the vein-coats also. These last, as they are weakened and inelastic as well as thickened, are very apt to give way and cause dangerous hæmorrhage, as is well known, in this kind of ulcer. The thickening of connective tissues and of the vein-coats is very well calculated to suggest to the mind of the reflective observer what the real cause of the disease is, for how can thickening or over-growth arise without over-nutrition? or, at least, what cause is so likely to produce it as over-feeding? Accordingly it is found that restriction of the diet has a very beneficial influence on this form of disease. Very

few callous ulcers of the leg indeed, with or without the accompaniment of varicosity of the veins, would fail to be greatly benefited, and many would be cured, by a fast of six weeks or so. A woman of 63 years of age whom I treated in this way, or by putting her on a pint of milk and half a pint of mutton- or beef-tea daily for six weeks (a greatly restricted diet, of course) was so much benefited that her ulcer of the leg of many years standing has never since that time given her any serious trouble. There is, in fact, nothing original in such a method of treatment, although it is not so often recommended as it ought to be, for in ulcer of the stomach patients are either put on a very little thin barley water or on say an ounce of milk and water every hour or every two hours during the day, and on a little water if they are thirsty during the night. But an ounce of milk and water every hour from eight in the morning till nine at night is only 14 half-ounces, or seven ounces, of milk in 24 hours, less than half a pint; and even 14 ounces of milk if undiluted milk were ordered (but if this were so it would probably be ordered not every hour but every two hours) would be a very restricted diet, which might, even without much strain on language, be called a starvation diet. Yet it is an excellent diet for ulceration of the stomach, and is very often prescribed by medical men. The same treatment is not so often resorted to for callous ulcer; but obviously the causes of its formation are the same, and it ought, therefore, to

be amenable to the same treatment, as in fact it is found to be by whoever has the courage to put it to the test. The reader may perhaps demur to the treatment of strumous or scrofulous ulceration by this means, but I have already mentioned the case of a child of four years old in whom I treated a strumous ulceration and suppuration of the knee by a greatly restricted diet, and this with perfect success, the child gaining about 10 lbs. in weight in the course of 15 months. This was a case of juvenile struma. Senile struma is also, I am convinced, much more amenable to a treatment by restriction than to the treatment usually recommended for it, viz., increase of the diet, as the following case will shew. A gentleman of 58 years of age, who, to use popular language, might be described as being riddled with strumous ulceration, affecting his back, groin, leg and arm, and who had undergone surgical treatment at the hands of a most distinguished surgeon without much, if any, benefit to his ulceration, had his wounds healed and his system strengthened by restricting his diet to practically one meal a day, continued for over a year. Another man, in the prime of life, who suffered year after year from suppuration of the glands of his neck, was entirely cured, and that without recurrence, by two meals a day. This last was a fine well-made man, about 5 ft. 10 ins. in height, and weighing about 186 lbs., who might therefore be supposed, and generally is supposed, to require a quantity of food larger than the average,

in order to keep up his frame and strength. Nevertheless, by reducing his usual three daily meals, and occasionally four, down to two a day, his ulceration was quite healed, and has not recurred.

The following case is illustrative of the beneficial action of the same principles. It was a case of (probably) ulceration of the large intestine. A woman, 54 years of age, anæmic, and whose appearance suggested even the idea that she might be suffering from pernicious anæmia, or some other possibly malignant affection, came into the Bradford Royal Infirmary suffering from frequently recurring loss of blood per rectum. The quantities lost amounted to about eight ounces at a time, dark liquid blood for the most part, with a few clots. The site of the bleeding could not be discovered. She had been under treatment for some time, but, beyond a diagnosis of ulceration, of probably the large intestine, a diagnosis which I believe to have been perfectly correct, nothing further could be said about it, and she had not improved. The source of the hæmorrhage might have been in the rectum, although the finger could not detect it, but probably was higher up in the colon. Of course it might have been higher up still in the ileum. In any case, here was hæmorrhage recurring in a thin, wasted, attenuated woman, and, evidently, unless it was checked, it must prove too much for the patient. To cut down on the bowel, first opening the abdomen and then the gut, on the chance of finding the spot from

which the hæmorrhage came, and of excising the ulcer, if one was found, seemed a very haphazard proceeding. If the surgeon failed in one place, he would have to stitch up his opening, and proceed to another attempt to find the bleeding-point. If his second attempt failed, he would have to make a third, a fourth, a fifth, and even an indefinite number of attempts to find the source of the hæmorrhage along a portion of bowel measuring eight feet long, even if it was in the large intestine, which was by no means certain. There can be little doubt that such a set of proceedings would have most probably cost the patient her life. Although, therefore, she came into hospital for operation, I refused to operate. The sequel is very interesting. Under a diet of half a pint of milk twice a day, and a little soup in the middle of the day, continued for six weeks, this patient made a gradual and finally a complete recovery. The quantity of blood lost fell at about the third week of treatment to six ounces, later to four ounces, later to about two ounces, and finally ceased. The intervals at which the bleedings occurred gradually lengthening between the attacks as the quantity of the hæmorrhage diminished, plainly showed also that the ulceration was gradually healing; and although we did not see the process going on, and did not even know exactly where it was, it was very easy to compare the healing of external ulceration which one had often seen, with the steps of the process which were occurring somewhere in the bowel, and

I do not now believe that our imagination was incompatible with the facts. The patient was recommended to eat only twice a day for the rest of her life, and when she left the hospital was quite well, the gray, ashy colour of her face being considerably improved. No doubt, if she does as she has been advised, or, if necessary, eats only once a day, she will recover even more than she has yet done.

The result of this case confirms me in the general view that ulceration is caused by starvation of tissue due to previous hypertrophy, and that hypertrophy, wherever found, is generally due to over-feeding. Of course, ulceration may be due to direct starvation, but this happens so rarely in civilised life, that we may practically throw it out of account. Practically it is almost always due to indirect starvation, not to direct. A sensible doctor will inquire which of the two opposite causes is at work, and will act accordingly, treating the effects of direct starvation by cautious increase, and those of indirect starvation by cautious restriction of the diet. The explanation of the mode or method of causation determines the mode or method of treatment, and seems to me to have been the same, in the case detailed, as has been described now more than once. The *essential* cause was too frequent and too abundant feeding (pollaki-siteism and poly-siteism). The *accidental* cause was too much bread and butter and tea, which formed the main part of the diet (pollaki-amylism and poly-amylism). Through too

frequent and too abundant feeding, first over-nutrition and then starvation through blocking of its circulation, took place at some unknown point in the gut. Ulceration followed, which opened a vessel or vessels, so causing the hæmorrhage. Under restriction of the diet, the exudation into the surrounding tissues was gradually removed, and the circulation in the place, wherever it was, gradually became re-established. By this means the ulceration was healed and the bleeding ceased, the gradual diminution of the quantities of blood thrown out coinciding with lengthening of the intervals between the attacks. For let us look at the argument. If too abundant feeding, and in women too frequent feeding (for how often do women eat because they are "faint"—not much at a time, but a little food and often !), is the cause of hypertrophy and exudation; if the exudation has a low vitality and dies, leaving a raw place, otherwise an ulcer—if these things are so, then it follows that restriction of the diet, or even starvation, will gradually remove the blocking of the circulation, which is only another name for exudation. In the case in question, restriction of the diet *was* followed by healing of the ulceration, or was, at least, followed by cessation of the hæmorrhage, which was probably due to ulceration. Will not any sensible man, who realises the force of the steps of this argument, and who realises besides the happy results of the treatment founded on the argument, be confirmed in his belief of the soundness of his

general conclusion, and be induced, therefore, to put into operation a similar chain of argument, should any similar case again come before him, and to follow it up by similar treatment? It is probable evidence, no doubt, not demonstrative, but it is the kind of evidence on which we act in conducting the practical business of life, and is quite sufficient for the successful conduct of that business by sensible persons. In accordance with such evidence juries and judges give verdicts which, if they are wrong occasionally, are usually correct, and give, on the whole, satisfaction to the public.

The uncertainty and flightiness of mental condition which so often accompanies the lymph congestion of the connective tissue of the brain and nervous system, and which, when aggravated, not infrequently sends women to asylums, is often accompanied by a sign on which I wish to say a few words. I mean dilatation of the pupils. Dilatation (like all? other signs) may be due to two opposite states, to stimulation of one set of nerves, the cranial, or to paralysis of another, the sympathetic. When a person becomes comatose with a serous effusion on the brain, his pupils are usually widely dilated. That is because both sets of nerves, the cranial and the sympathetic, but particularly the latter, are paralysed or have lost their power, The transverse elements of the pupils are governed by a cranial nerve (the third), while the circular fibres are under the control of the sympathetic. Stimulation of the former, therefore, dilates the pupil,

while stimulation of the latter contracts it; and of course, complete paralysis of both, by relaxing both, causes the dilatation that threatens, if it does not necessitate death. Now in the flighty and uncertain state of mind to which I am referring—it is often called brain-fag—when the connective tissue of the brain is congested with lymph (when cerebral initis is present), there is also very often present initis of the third cranial nerve, and hence over-stimulation of the same, and of the fibres in the iris which it supplies, with consequent dilatation of the pupils. Sometimes this expresses itself in headache rather than in mental disturbance, but however brought about, excessive pupil dilatation ought to attract our attention, as it is a sign that there is too much lymph about the base of the brain, too much lymph depending on too much or too rich blood, and too rich blood depending on too much food. Sometimes, indeed, this condition leads to defects of vision in young people, the proper remedy for which is rather to recommend a restriction of the diet, than to try to find spectacles which will overcome the defect. I have several times in fact been able to overcome visual defects, especially in children, by altering their diet, the visual defect being rather the local manifestation of a general condition than the substantive affection. In children the sign often accompanies a too slow pulse (48 or 50, *e.g.*), and both the slowness of the pulse and the dilatation of the pupils are to be met by restriction of the diet, as in fact both have been caused by excess of food.

In young neurotic women also we frequently find dilatation of the pupils, although the pulse may not be disturbed, or, if it is, it may be too quick rather than too slow. The sign is not so often found in men, although the slow pulse often is. I can only explain this by saying that, as a rule, men have a greater resistance than either women or children, although no one knows better than I that this is no explanation, and that it is merely a statement of the fact in another form. However, I think it is a fact. What, however, I wish to draw attention to is the highly important practical conclusion that dilatation of the pupils (whether accompanied or not by the changes in the pulse rate referred to) is best combated and ought to be combated by restriction of the diet. And I have to add that in a few weeks (two to six, for unfortunately it takes a rather long time), under a restricted diet say of a pint of milk daily, divided into small quantities at intervals, these dilated pupils can be restored to their normal state. I could give cases in which this sort of management has been quite successful, but space forbids.

There is, however, an important disease in which this sign of over-dilatation of the pupils is frequently present, on which I wish to say a few words, and that is epilepsy. I have seen first and last a good many cases of epilepsy, and have come to have a strong opinion that its causes also are chiefly wrong feeding, and particularly too abundant feeding, acting upon a low resistance to digestive

labour. It is very difficult, no doubt, to prove the truth of this view or of any view, in the case of epilepsy, for this reason: Epilepsy has a very varying and variable periodicity. In some cases the epileptic fits come frequently, one or two or three (for they often occur in "bouts" or sets) taking place every few days. More often they are separated from one another by weeks, sometimes by months, not infrequently by years. And the next fact is that the occurrence of the fits is not regular. If there have been two or three sets in one week, or in a month, it does not at all follow that there will be a like number in the next succeeding week or month; and the same statement is true for longer periods of time, months, for example, or years. Now if the last fit or set of fits occurred six months ago, or a year ago, and if the times of the occurrence of the fits are not regular, how are we to know or to judge what the effects of our treatment have been? If a longer interval than formerly occurs under treatment, it may have been because the fits were not going to return in any case, and the longer interval of freedom may not, therefore, be due to our treatment, but to Nature. No doubt it was chiefly for this reason that the ancients called epilepsy the sacred disease, and allowed the priests, rather than the physicians, to deal with it, allowing their claim to be divine agents if they seemed to be successful, and if the patient seemed to recover; and if they failed, attributing the failure to the belief that the disease

was a sacred one and in the hands of the gods only, who in this case would not allow it to be cured. The same kind of reasoning led others of the ancients to think that the disease was due to the patient being possessed by devils, and that only those could cure it who had the power to cast the devils out. It is certainly a little curious to find that the remedies of fasting and prayer were greatly valued in managing epilepsy, because, certainly so far as I have seen, far and away the best remedy consists of a careful restriction of the diet, while the mental calm implied in the act of submission which prayer involves is a great adjuvant to the cure. And the mode in which the remedy acts is plain enough. The connective-tissue of the brain being congested with lymph, restriction of the diet compels the body to draw on the over-accumulation of its resources, and to convert them gradually into the heat and motion of life, the consequences being gradual, or rather intermittent, relief to the irritation which caused both the dilatation of the pupils and also the explosions of the fits. The transverse elements of the pupils are, in fact, in a state of cramp, as it were, which is slowly or intermittently relieved, just as cramp in the legs can be relieved by cautious diminution of the diet. For these reasons, and following out this suggestion in treatment, I have for many years now advised restriction of the diet in epilepsy to two meals daily, and sometimes to one; and in acute cases have recommended further

and great restriction to a pint or a pint and a half of milk daily for a considerable period of time, some weeks, for example, and that with excellent results, so far at least as results can be guaged in a condition which is so irregular in its exacerbations and recessions. Especially are these results good in the case of children, although older persons are also often very amenable to the same line of treatment. Fasting, in fact, seems to be of very great efficacy in the treatment of epilepsy, the rationale of its action being what I have attempted to make clear to the reader.

As has been said, dilatation of the pupils is seen in many other conditions besides epilepsy. "Neurotic," or what I have called "initic" women, full of all sorts of ailments, often manifest it, and children also, who sometimes concomitantly manifest defects of vision. It sounds absurd and far fetched, until we see its rationale, to say that defects of vision can be remedied by alterations of the diet; but I have literally seen this brought about in more than one case, having been able to correct short-sightedness in children, for example, by restricting the diet, and this without the use of glasses, when the cause of the defect was pointed to by Nature by over-dilatation of the pupils through over-stimulation of brain and nerves, by excess of food in the way detailed. When, by diminishing the food, irritation and over-stimulation were reduced, the over-dilatation of the pupils was reduced also, and the accompanying defects of vision relieved.

Over-dilatation of the pupils, one or both, is, in fact, a sign of such significance that we can often, by paying attention to it, foresee and so sometimes prevent the onset of an attack of inflammation of the brain (cerebritis or meningitis) in children or young people, and by a timely restriction of the diet save and prevent damage to young lives.

The considerations advanced in this essay, the view that most of our diseases are brought upon ourselves by our own folly, by too great gratification of our appetites, and the consequent inference, so often verified by proof, that most of our diseases are tractable by restriction of the diet, have come to exercise a kind of fascination on my mind. And other considerations suggest themselves also to one's mind, conclusions, for example, as to the uniformity of nature, and the order and government of the scheme of things under which we live, which, though somewhat out of place in an essay on medicine, are yet of the deepest and even most entrancing human interest. Nature has given us, for example, with a bountiful hand, a large variety of good things to enjoy. On the other hand she has endowed us with the desire to enjoy them; but she has left us to discover by observation and experiment what are the effects of free indulgence and of restraint. And it is for each of us to say what he will do, how far he will go, and what amount of government he will attempt to put upon himself during his progress through life to

death—an infinitely greater consideration than the far commoner one, how much influence he may hope to have over his fellows. But these considerations and the analogous one that thinkers have held that all our knowledge is always a mean between two extremes, neither of which is possible, must be left aside by the medical man, who yet is at all points compelled to conclude that neither ascetic abstinence nor florid (nor yet anæmic, nor tripthæmic) plethora is healthy; if, that is, as seems to be the case, the aim of medicine ought to be to make life fit for the active work whose performance itself brings happiness and helps to render life free from disease.

On the other hand, a certain amount of depression is apt to arise when one reflects that comparatively few appear to see so simple a way out of their troubles, as that to be got through control of our appetites, and that of those few, only a small minority seem willing to put their discovery into practice. In every generation, however, a minority of medical men have apparently said much the same thing as the present writer. A most remarkable instance, as we have seen, is Hippocrates in the ancient world. We may also instance Celsus, who said, *potius bis quam semel in die edere*, for although he thought it better to eat twice a day than once, he certainly did not countenance the four or five daily meals which, as we have seen, characterised the daily life of the Roman citizens in the days of the wealth and luxury which preceded and caused their decline.

In the time of the English revolution also, the great Sydenham tells us how he fed a gouty man on milk and whey (*serum lactis*) for a year; and just before the present time Dr. King Chambers spoke of the starvation of over-repletion, to be translated into yet more idiomatic and expressive language, when Dr. Dewey speaks of the starvation of over-feeding. In his day, Sir Andrew Clarke was called the starving doctor; and it would be invidious to mention the names of a few living physicians who make like deliverances. How will the view, the view expounded and defended and illustrated (may I say proved?) in this essay—how will it fare in the future? Will it be accepted or rejected? Or will it, perhaps, be accepted as a general proposition, or as a proposition generally true, but only to receive practical rejection when the attempt is made to apply it to any particular case? It is so easy to see the general law, but to fail to see its application to our own case. Better, it seems to me, would it be to denounce it out and out than to accept it in a half-hearted and insincere way. I have often thought what an interesting essay might be written on the medical history of great men, and how it might be shewn that their food habits determined not only their longevity, but many of their moral qualities also. But this task would be too great for the termination of our exposition, although, perhaps, it would be as interesting as any other part of it. I have already mentioned the great man who wrote the vision of Mirza, as a striking

instance of those who say and do not do. But many others probably do not have the opportunity of knowing what he knew, and so of shewing whether they would be willing or not to obey the law in their own case. Illustrations of the lamentable effects of ignorance of the law abound in our literature to all those who have an eye to read and see them; and a noteworthy instance is afforded by the medical history of a well-known literary character of the truth of the statement that too frequent and too abundant feeding cannot be borne by average humanity without continually recurring illness and premature death. Of course, if the law is the law, and not a mere figment of the imagination, it will be found to cover all cases belonging to its own order, and when exceptions occur, some peculiarity in the circumstances must be looked for to account for them. This literary character has been depicted from her own letters most fully for our instruction, and it appears that from somewhat early days she suffered from recurring headaches. At a very tender age, we are told, she suffered greatly from cold, and found difficulty in getting near the fire at school in winter to become thoroughly warmed, owing to the circle of girls forming round too narrow a fireplace. The biographer is not a physiologist, but describes in wonderful detail, although nearly always by reference rather than description, the ailments from which his subject suffered. The incident relating to the fire occurred at five or six years of age. If

only the feeling of cold had been met by judicious exercises and a wise regulation of the diet, in place of the futile attempt to get near the fire, if the oxy-uri-chæmia causing the feeling had been treated in place of the feeling itself, how different might not the result have been! For the idea of the child's father was the conventional, and perhaps natural, one, till modified by fuller knowledge, that a child's strength was to be promoted by increase of food, since we find that at about thirteen years of age, at her next school, "a source of great interest to the girls, and of envy to those who lived further from home, was the weekly cart which brought the child new-laid eggs and other delightful produce of her father's farm"—these new-laid eggs and delightful produce of the farm being, no doubt, taken as extras in addition to the already, probably, quite sufficient school fare. By the time the child was nineteen or twenty her headaches were in full swing, frequent references being made to them. Thus, at twenty years of age, she speaks of hardly knowing herself, owing to the insuppressible rising of her animal spirits, "on a deliverance from sick headache." Evidently she suffered from recurring sick headaches. If only she had been told that when persons suffer from recurring ailments they should look for a constant cause, or a frequently acting cause! Had this been done she would have had the option of choosing whether she would accept the unpleasantness of putting upon herself a certain amount of restraint,

or of going on in the old way and suffering from the recurring ailments. She would have had the opportunity and responsibility of choosing whether she would belong to those who know or to those who do not want to know. This heroine was exceptionally gifted—most exceptionally so—and saw, therefore, though without stating it, or perhaps without realising its significance, that alternation which always goes on, and always must go on, in the body as to *strictum* and *laxum, τάσις* and *χάλασις*. "Have you not," she says, "alternating seasons of mental stagnation and activity?" More mental power and less in the waking state, that is natural and healthy; but "stagnation and activity" have transcended the limits of health, and are as morbid or unhealthy as panic and boom in business or as depression and fever in the body. The ill-health goes on, for at twenty-seven years of age she speaks of a cold and headache as being doubly intolerable; and at thirty she has headaches and backaches. At the same age she is suffering "as acutely as ever I did in my life." But a little further on, and at the same age, when she had gone abroad, we have described to us, though all unconsciously, the chief predisposing cause of these ailments and delicate states, which were thought to be the effects of hard work, of anxiety, of "exposure to cold," of a bad, miserable, foggy climate, etc., in a way which is so instructive that it must be quoted. "I breakfast in my own room at half-past eight, lunch at half-past twelve, and dine at four or a

little after, and take tea at eight." If one had been in search of a plan to induce ill-health and early death, one would have adopted that of breakfasting, lunching, dining, and having tea within eleven and a half hours of one another. If anything additional were wanting to destroy life, it would have been what some persons of my acquaintance do, viz., to have some milk and fruit brought up to their bedrooms, in case they should feel "faint" in the night, or to make a point, as some of them have done, of rousing the maid in the night to heat milk or make tea and bread-and-butter for them. In another place she tells us that she is becoming so thin as to be approximating to length without breadth ! Was that due to under-feeding ? Is it likely that a person taking four meals a day would suffer from direct starvation ? How is it possible ? Was it not, rather, the starvation of over-repletion, the attenuation of excess ? No doubt it was. And the proper indication would have been to diminish or restrict the diet, not to increase it. The action of the predisdosing cause continues. We find her describing herself as suffering from constant colds. No sooner is one passing off by its stuffy stage than another is beginning with its wet one. Writing to a friend, she unwittingly informs us of the cause of this also. "You know our habits," she says ; "lunch at half-past one, walk from two to four, and dinner at five." How could any person hope to be well who followed such pollaki-siteous habits ? Headache succeeded headache, cold

succeeded cold, backache and rheumatism tormented the patient ; and constant change of air and scene, with frequent runs to the Continent, were adopted to get rid of ailments which were not due to climate at all, but to wrong living. And as the causes continued their action, how could the effects cease ? But why say more ? Alas ! that noble life came to an end at sixty-one years of age from disease of the kidneys, intercurrent attacks of headache and of rheumatism sadly interfering with work ; and the world was deprived of the maturer sagacities which would no doubt have flowed from her pen as her age advanced. Had she overcome the physical weaknesses of humanity, she would have become even purer and more ennobled, reflecting in advanced age most clearly the transforming light as from a mountain-peak, ascending ever higher and nearer to the heavenly source of knowledge and inspiration. The hard work, the anxiety, the cold forbidding weather, the fog, and the other drawbacks of an English climate (the healthiest, probably, under heaven, notwithstanding), would have failed to destroy the life so early, had the foundation of it, the nutrition, been put on a proper basis. And how simple the remedy ! Breakfast at half-past eight and dinner at four or a little after, with the omission of the lunch and the meal at eight (though a cup of tea alone might then have been taken if desired), such a plan would have interfered with no one's comfort, would have reduced labour to others and not increased it, would have led to the disappearance

of the headaches, the backaches, the colds and the rheumatism, and would have postponed probably for twenty years the onset of the kidney disease which so prematurely ended the life. Persons are so apt to say that they eat too little at a time to allow them to adopt such a plan, just as if they could not trust to Nature to see that they have enough, and forgetting or being apparently unwilling to see that the demands of the body are much less than their preconceptions declare, and that fine literary work or pressure of anxious business is certainly not to be met by poly-siteism and pollaki-siteism. How many literary lives of the finest order and how many business lives of the highest calibre have been destroyed in this way twenty, thirty, forty years before their time, and how many delicate and refined mothers have left their children motherless from the same cause !

I have now mentioned a considerable number of kinds of disease which affect humanity that are amenable to dietetic treatment, that can be prevented by proper modes of living, and particularly by proper dietetic modes of living, and that can be cured, if they have not advanced too far, by proper dietetic measures. I have shewn that these considerations are true for bronchitis, for broncho-pneumonia, for asthma, and for pneumonia; for rheumatism and gout; for neurosis and hysteria, and brain-fag, neuralgia, and even many forms of insanity; for the fevers and the specific inflammations, like erysipelas of the face, and sycosis of the chin and

cheeks; for senile and juvenile struma and scrofula; for ulceration and for other forms of disease. I have shewn that the cause or the main cause of all these various forms of disease being improper feeding, they can all be prevented and even cured by having recourse to proper feeding. And the curious and simple consideration has been forced on our attention that the particular form of improper feeding, which is causing so many of our diseases, is not too little food, but too much. So that the cure in all cases, or at least in so large a majority of cases as practically to amount to all (say two-thirds to nine-tenths of all cases of illness), is not to increase but to restrict the diet. Still more curiously and more simply does it seem to arise from what has been said that if we were to feed our children and ourselves, living in towns, on two daily meals, to the extent of say an ounce of mixed diet daily for each ten pounds of our body weight, or perhaps in the case of the children an ounce daily to each eight pounds of their body weight, we should obtain on the average very much better health than we do now. Do we suffer from recurring attacks of bronchitis, broncho-pneumonia, pneumonia, or asthma, then let us see what dissiteism, to the amount of about a pound of mixed diet daily will do for us. Do our children keep taking colds, or catching fevers, getting enlarged tonsils, or adenoids in the nose? Let us see what we can do for them, after their recovery on a fluid diet, by putting them on two daily meals, consisting of say

an ounce of mixed diet daily for each eight pounds of their body weight, or on three meals at the outside. Are they suffering from juvenile struma, or are the seniors among us suffering from senile struma? Let us put them on two daily meals, and the seniors even on one, and they will recover from these serious evils more quickly and more safely than if treated by any other means. Or do we find ourselves suffering from specific inflammations, like erysipelas of the face, whether associated or not with the growth of micro-organisms in the blood and tissues of our bodies? Let us see what effect two daily meals will have on us and our ailments, the quantity of our food being regulated as suggested. After 50 years of age, let us reflect that we are safer with one meal than with two, that monositeism is better for us than even dissiteism, but that it is at least much safer than tris-siteism or tetra-siteism, and still more than any larger number of meals. Do we find ourselves suffering from ulceration anywhere in the body, between the toes, for instance, or on the face, or in any part of the skin, or from eczæma? Let us see what the effects of the same treatment will be, with perhaps some soothing topical application to the affected part. Truly the re-discovery that disease is one, one in causation and one, therefore, in treatment, one also in the means to be adopted for the prevention of its very various phases and forms, this re-discovery is at last put to simple and very practical and efficient use. The question, in fact, arises: are there any diseases

to which these principles are not applicable? We saw, as regards cancer, not, indeed, that we may have much prospect of cure in cancer from dissiteism or monositeism, or even a-siteism, but that under fasting and starvation its pain is much diminished and its suffering assuaged; while by a proper system of dieting there is the strongest reason to believe that this terrible scourge could be prevented from attacking humanity, and from sweeping away its most useful members in the very culmination and acme of their activity and powers. Are there any diseases to which the same principles are not applicable? I do not know of any. The considerations advanced in former chapters, dealing with the alterations of function, and shewing how health and disease shade off into one another by insensible gradations, also shewed that diseases of the heart, of the circulation, of the brain, of the respiration, of the nerves, of the intestines and of the kidneys, are all due to changes in nutrition, and depend more on digestion, and therefore on food supply, than on any other one cause, and probably more than on all other causes put together. What other diseases are there which are not amenable to this consideration? Appendicitis? The diagnosis is too pat in most cases. It is not the appendix which is inflamed in so many of these cases, so much as the cæcum or typhlon, that is, the part of the bowel to which the appendix is attached. We have generally to do with a peri-typhilitis, or an enteritis, or inflammation of the bowel, rather than

with appendicitis. And not infrequently in what is called appendicitis, the connective tissue of the pelvis and of the muscles of the abdomen is inflamed also, the suffering child wincing like the neurotic or hysterical woman, so called, when you press him even lightly on any of these widely distributed tissues. Are we to remove the typhlon or the cæcum or the gut itself to cure inflammation of the bowel? Or the connective tissue or muscles of the abdomen to cure appendicitis? Are we to amputate the head in order to cure neuralgia of the face? Are we to consider that the highest aim of medicine and surgery is to remove organs and structures with which nature has endowed us? or is it rather to advise us how to live, so that we may be able to retain them in a sound and healthy condition? Truly the advances of surgery in this generation have been great. But is there not too much surgery? What if much of it could have been made unnecessary, and what if much impending surgery for the future may be rendered unnecessary by proper methods of living? Would not that be much better than boasting of what we are able to achieve in the way of removing organs and parts from these wonderful bodies of ours? If the duration of life has been increased, how is it that the sickness is greater than it was? Is it not because in our worship of wealth and luxury we have forgotten moderation, and are over-looking the necessity of exercising that amount of self-restraint and government, without which we fail in

the noblest aims of humanity? Yes, it is hard for the children to have to face this question. I freely admit it. Their elders do not find it easy, but sooner or later all of us have to face it. But truly the penalty for breach of the laws of nature is not averted by any frantic appeal of ours, from the tender young or feeble old; nor can it even be made to fall, much though we might desire it, only on the mature and on those who are or ought to be most able to bear it. Therefore, it seems to me, even the children must be taught, and that early, what is the nature of the law and of the government (merciful and gracious as it seems to me) under which we live. And if instruction is thus early imparted, the feeble old will not be called upon to bear anything more than their strength will allow.

Now I daresay it will be easy to bring ridicule to bear on what has been said. It will be represented that monositeism and dissiteism are held up as a panacea for all the ills of humanity. I know as well as any omniscient critic that there is no panacea against suffering. Do what we will or may, there will always be a great deal of it. *That* also has its lessons, with which I do not pretend to deal. But if any man can tell us what better remedy (even if there is no panacea) there is for the evils of life than moderation in all things, whether in work or rest, in food or drink, in joy or sorrow, in egoism or altruism, he will do well to declare it. And if he can offer a better

approximation than mine towards the definition of what moderation is, no one will be readier than I to consider and adopt it.

CHAPTER XIII.

The Effect of Exercises on the Body.

I HAVE said very little hitherto, beyond the reference made in discussing Mr. Smith's letter in the last chapter, on the effects of exercises on the human body. By implication, indeed, I have said a good deal, or at least the reader has been left to infer a good deal. Dr. Dewey, with the fine medical insight that characterises him, thinks that exercises are not particularly indicated for the preservation of health, in this agreeing with some ancient authorities, notably Erasistratus, who said much the same thing. I have already mentioned my own agreement with both of these authorities, provided that people in general took no more food than they require in order to keep the body in health and efficiency. But as most, and in fact nearly all of us do fall into the latter mistake, I still agree with the majority of the ancient and modern physicians in recommending moderate exercises to be taken, especially in the act of dressing in the morning, in order to keep the body supple, young and active, and in order to render ourselves more efficient for the work of the day. But I think I ought to enter a *caveat* about their

use. For if a man persistently over-fills his blood and connective tissues with material ingested greatly in excess of his requirements, he will find himself suffering particularly in two disadvantageous ways. In the first place, he will find himself very much disinclined for exercises, and secondly, when he takes them, they will seem to tire him instead of freshening him. He may even do himself harm by them, especially if they are violent or severe, or too long continued or too suddenly entered upon. I do not, for example, think it is at all a wise thing for a man engaged for weeks or months in somewhat sedentary business pursuits, to go out for a long day's shooting, involving heavy exercise or laborious tramping over rough ground. If he has been over-working his digestion for a long time, and if he then adds to his overwrought organism the labour of action and sustained exercises, to which he has been for long unaccustomed, he will simply damage himself, because he will be over-working his body in two directions. He *might* possibly be able to sustain the labour of one of these (though of course even that would not be wise), but the combination of the two will be almost certainly harmful. The effects are a little like those of both eating and drinking too much. It is most unwise to do either, but to do both together is certainly more damaging than to do one alone. And so with exercises and over-feeding or over-drinking. The business man, therefore, who wants to go shooting, had better take say a couple of hours' exercise of

that sort the first day, and go for a forenoon or an afternoon on the two or three next succeeding days, before he ventures on spending the whole day in work to which for a long time he has been unaccustomed. And the same general rule ought to govern the taking of any kind of exercise, as riding, bicycling, cricketing, football, &c., it ought to be entered on gently, and gradually increased. There can be no doubt at all that much damage has often been done by neglect of this rule. Of course, for the countryman, accustomed to ride or to do heavy manual labour, the same precautions would not be necessary. But I am advising townsmen for the most part; or country people, who, although they may live in the country, are yet occupied in what may be considered as townsmen's work; and it is these whom I have in my mind, as indeed they form the large majority of the people of England (and most other civilised lands) to-day.

The subject of exercise has hardly, I think, received adequate consideration. True, the public school education of England has always laid great stress on the value of games and exercises, the influences of which have been most beneficial on the health of her boys and young men. But until comparatively recent times this means of development has been almost wholly neglected in the case of girls. With them, as with grown up women, it used to be assumed that the performance of the business of life would provide sufficient exercise for the various muscles of the body. At

least, nothing was so common, when exercises were recommended for young or old, as for the medical man to be told, "Oh, I am moving about from morning till night. I cannot require exercise. If anything, I have too much." But the muscular movements involved in the performance of the business of life are very often monotonous, and involve the performance over and over again of the same, and that a limited number of movements or of exercises. Take, for instance, the life of the woman doing domestic work. Her occupation is varied enough, certainly. To be one's own cook and housemaid, and charwoman, and nurse, and laundress, all at once, at short and alternating intervals, seems to imply the need of making a large variety of movements. And yet, even with all these, certain muscles hardly get exercised at all. How stiff many of the women, so occupied all day long and day after day, become in the muscles of the back, for instance, is well-known to those who have to advise them. Of course some of these conditions, as well as the *peliosis* from which so many women suffer, are due, as has been said so often, to the wrong food habits in which so many of them indulge. Still some of the effects are attributable to confinement indoors, and to the absence of methodised exercises. In fact, such women suffer from three great hurtful conditions—too frequent eating, absence of fresh air, and want of methodised exercises. We have seen how the first cause over-fills the connective tissues of the

body with lymph and lymph-corpuscles; and it is easy, therefore, to see how, before this has culminated in its effects by acting over a very long time, these effects can be combated at least to some extent by well devised exercises, since these stimulate the circulation and lead to the oxidation and combustion of some of the unused stuff accumulated in the body. It is an interesting thing, also, that the performance of methodised movements for ten, fifteen, or twenty minutes, twice a day, not only does not add to the fatigue of persons so occupied, but positively diminishes it. and enables them to do their daily work better. It is better to rest a little after these movements; but if, for instance, they have been taken in the morning, finishing the act of dressing usually gives all the rest required, and, after a drink of hot coffee or tea, one is stimulated for the vigorous entrance on the work of the day, which will be done effectively, as, like a spring compressed and gradually uncoiling, the body gradually gives out the energy received by sleep and from the food of the day before, in the form of heat and work done. Few persons are so occupied that they cannot spare the time required for such exercises. What sort of exercises? The ancients appear to have used two main sorts, what they called *gestatio,* and what was called *exercitatio;* or what we should now term passive and active movements. On the whole, a good deal of importance seems to have been attached to exercises by the Roman physicians. I

do not suppose for a moment that their advice would be followed by the mass of the people. The social condition of the population would act as an effectual barrier to that, as unfortunately it still does to a great extent to the mass of the people of our own day. But we find Celsus writing after this fashion. "He whom either domestic or civil duties have occupied during the day ought to set apart some time for the care of his body *(curationi cerporis sui)*, and his first care is exercise *(exercitatio)*, which ought always to precede food. And exercise ought to be more elaborate *(amplior)* in the case of him who has worked little and has been well supplied with food" *(bene concoxit*—it is really surprising how much these ancient physicians knew) "while it should be less in the case of those fatigued with labour, and who may have digested less."

As to passive movements, "the most gentle gestation," says Celsus, "is that of a ship, either in a harbour or a river; the more violent is on the high sea or in a suspension couch (*lectica*); more violent still is that of a carriage. And indeed each of these may be either intensified or rendered more gentle. If there be none of these things within a man's reach, a bed ought to be suspended and moved from side to side. If there be not even that, a prop is to be put under one foot, and by this fulcrum the head is to be propelled backwards and forwards by the hand."

A rather strange, but certainly interesting

picture, this, of the passive movements which were prescribed for patients in the second century of the Christian era! It is to be hoped that they were beneficial. The active exercises were, on the other hand, "reading aloud (*clara lectio*), the use of arms, the ball. Running and walking are," he says, "very convenient exercises; the latter of which would be more advantageous if not on a plane, since the body may be exercised better by an ascending and descending variety, unless it be very weak. And it is better in the open air than in a portico; better in a shade formed by walls and shrubberies than that which is under a roof; a straight walk is better than a winding. But incipient perspiration ought generally to terminate the exercise, or at least lassitude short of fatigue, and even in this, itself, it ought to be sometimes more, sometimes less. And, indeed, there ought to be no fixed rule (*nec certa esse lex*) nor immoderate labour in these exercises, in imitation of the athletes. Unction very properly follows these exercises sometimes, either in the sun or before the fire; at another time a bath, but in a chamber as high, light, and spacious as possible. . . . . After these things it is necessary to rest a little."

The elaboration of the bath also is well known to have reached some perfection among the Romans, with its *tepidarium*, *calidarium,* and *laconicum,* its *solium, piscina,* and *frigidarium*; besides the adjuncts of the *aquarium* or reservoir, *vasarium,* or

place for holding the vessels where the water was heated ; and the *hypocaustum* or stove.

If we set aside the somewhat fanciful arrangements and advice as to passive movements (*gestatio*), I think we must admit that the ancient physician who recommended the arrangement for active movements (*exercitatio*) knew well in this matter, as in so many others that he wrote about, what he was talking of, and gave good advice. He may not have known anything about oxidation, or about the accumulation of various waste products in the connective tissues of the body. Still less could he have named those products ; but his juxta-position of digestion (*concoquere*), with movements (*gestatio* and *exercitatio*), and his clear perception of their interdependence and relations to one another, mark him out as possessing the insight required by the accomplished physician of all times. It is the same man who formerly compelled our admiration when dealing with the pulse and with the allowances which ought to be made for changes occurring in its rate and rhythm, and that, many hundreds of years before the discovery of the circulation of the blood.

As to the exercises themselves, the reader may ask, of what sort they should be. They should be arranged so as to move in a methodic manner all the muscles, not, as is too often the case, to move some muscles to the exclusion of the rest. Games are, of course, most useful for this purpose. The games ought to be such as are not too violent. For

young people, of course, more freedom and rapidity of movement, and more exertion are allowable than for middle-aged or elderly persons. Cricket, which involves both the action of running, and the many movements of throwing, catching, striking, and finding the ball, is, of course, an admirably contrived set of exercises. Football also might perhaps be so, if it could be relieved of its present drawbacks. A game which involves wrestling and struggling so fierce as to wrench, and strain, and sprain muscles and ligaments, fracture collar bones, shoulder blades, ribs and legs, and do other grievous bodily injury, may be a good game and a well devised exercise in itself, but it is in need of reform. Lawn tennis also is a very good game, its few drawbacks being comparatively easily overcome. Golf seems to be in all ways a most admirable game ; and no doubt there are others, as billiards, &c., which are suitable. Walking the deck on shipboard has a fascination for many people, providing exercise in the open air with the least possible fatigue, and having the advantage that it can be stopped at any moment that fatigue is felt, and can be as readily resumed. Walking, running, leaping, fencing, boxing, the use of the rings and parallel bars, and the various movements involved in the use of arms, are all useful, and of much benefit to those who can indulge in them. Mention should also be made of the bicycle and tricycle now so much in vogue. No doubt, most valuable exercises can be obtained by their means,

particularly by women, whose lymph and blood circulation can often be stirred up in such a way as to materially improve many of those chronic initic invalids, whose cases are at once the despair of the medical profession, and the cause of so much distress and despondency to patients' friends. No doubt it is in wrong food habits, and in the absence of exercises that the causes of these depressing ailments are to be found; and the bicycle offers a means of supplying a useful form of the one necessity, while by stimulating the digestive powers to greater activity, it will also help to supply the other aid to healthy life. Of course all good things must be used in moderation. Already there are whispers of the overuse of this form of exercise (as from time to time there are also of others). It is devoutly to be hoped that abuse of a form of exercise, very good in itself, may not justify, or seem to justify, a denunciation, which, even if merited by cases of abuse, will be likely to restrict its proper use. There is everything to recommend the prescription of the use of bicycle exercises (and this applies also to all exercises), for such lengths of time as patients can bear, whether for half an hour, or an hour, or two hours, according to the needs of the particular case. The marks of a well balanced mind are that we should use things which are helful to life in a more or less equal and equable manner, with variations of less and more, indeed, according to the varying circumstances of the moment, and according to the varying needs of life. But, on the other

hand, we should avoid those extremes of rush and torpor, of too excited use alternating with unmerited neglect, of too enthusiastic recommendation and too vehement denunciation, which are the characteristics of want of balance in the mind of the individual, of the community, or of the medical profession.

All these modes of exercise, however, require much time, and many of them involve more expense than can be afforded by the average man and woman. Happily the board schools now train both boys and girls in movements and exercises admirably adapted to health. In later life, most men and women soon find that the main object of life is not amusement, but work; and it too often seems more or less of a satire to advise those who are ill to resort to out-door exercises. However admirable the advice is in itself, it is too often impossible to follow it; and indeed, had it been possible to adopt and adhere to it, the patients might in many cases have escaped being ill at all. But there are few persons so poor, or so much occupied, as to be unable to afford to give five, ten, fifteen, or twenty or thirty minutes to methodised movements twice a day, movements devised for the exercise of all the muscles of the upper limbs, of the head and neck, of the ribs and trunk, of the abdomen, of the flanks, of the thighs and legs. And everyone who wishes can suspend a pair of rings from a beam in the roof, or can use light dumb bells (they need not weigh more than two pounds each), or clubs, even

if he cannot find time or means to visit a gymnasium regularly, Of course the society found at a gymnasium adds a healthful stimulus to life, and may be recommended to all who can make the opportunity to visit one. For those whose time is limited, or who for other reasons cannot use gymnastic instruction, nothing could be better than to read the little book, say, of Dr. Schreber (Leipzig, Fleischer—it has lately been translated by Day, of Norwich) entitled "Aërztliche Zimmergymnastik." This little gymnastic instructor, "made in Germany," in its 25th edition, when I last saw it (the last edition consisting of 10,000 copies), gives wood-cuts of a large number of muscular movements and bodily positions, most admirably devised, and as pictorially instructive to those who cannot read German as to those who can. It says much for the sound sense of our German friends that there should be among them so great a demand for so useful a book, and offers an example which all would do well to follow, both in this country and elsewhere. I might also mention the chart published by Professor Dowd, of New York, as offering an excellent pictorial exhibition of useful movements.

There are also other valuable guides to methodised exercises. Especially does attention seem to have been bestowed on these in the United States of America. The foundation of such literature in modern times is, of course, the Swedish system inaugurated by Ling. But as a development

on this we have the handbook of school gymnastics, by Baron Nils Posse (Lee and Shepard, Boston), a small but well arranged set of progressive exercises for children. Then there is the physical culture of Louise Preece (C. W. Bardeen, Syracuse, N.Y.), which deals not only with systematic exercises and movements, but also to a considerable extent with the expression of the emotions by gesture. And a little book by Edwin Checkley, entitled "A Natural Method of Physical Training" (London: 24, Bedford Street; Strand; and New York: 27, West Twenty-third Street; G. F. Putnam's Sons), ought to be mentioned. Then there are "Sound Bodies for our Boys and Girls," by William Blaikie (London: Sampson Low, Marston and Co.), and "Modern Gymnastic Exercises," by A. Alexander, F.R.G.S., both of them very good books. All of these works give valuable information regarding exercises, and the methods of performing them, and may be consulted with advantage. And no doubt there are others, as Sandow's and Whiteley's Exercisers, which are also useful.

With these representations before one, it is possible to avoid the verbal descriptions which would take up so much space, and which would not then be so clear as the wood-cuts themselves. And I can go on, therefore, to say, respecting muscular movements, that the best time to take them is before a meal, as Celsus says, or *immediately* after, before digestion has fairly begun; otherwise it is not wise to take them say a couple of hours

after food, as then digestion is in full swing, and the economy ought not, so to say, to have its attention disturbed when engaged in the digestive process. Four or five hours after a meal, on the other hand, or an hour or so before the next one, is a suitable time, as the movements, especially if aided by the drinking of half a pint of water, stimulate vitality, increase oxidation, improve the power and rapidity of the circulation for the time, and stimulate the digestive viscera to complete the digestion of the previous meal. Another thing which these movements effect is, by stimulating the power and rapidity of the capillary circulation, to prevent the deposition in the connective tissue of those waste products which are apt to be filtered out of the blood at that period of digestion, and even to remove them if such deposition has already occurred. In this way, neuralgias, "brain-fag," rheumatisms, &c., tend to be prevented, and even cured. The best time for the taking of exercises seems, therefore, to be in the morning, in the act of dressing. The proper use of them may involve the getting up in the morning twenty or thirty minutes sooner than usual, not a bad suggestion for any of us. I think movements may with advantage be had recourse to once again in the course of the day, at some convenient time, as the organism does not seem to retain the impress of actions of this sort for more than twelve hours. Of course once a day is better than not at all; but to parody the words of Celsus about food, it seems to me that it is

better to have exercises twice than once a day, *potius bis die quam semel exercitationem capere.* If this be admitted, the best time for the second set of exercises will be about four, or five, or six o'clock, according as people have more leisure time. The well-to-do have often a toilet to perform at that time, and a cup of afternoon tea taken then will stimulate and aid the effect of the exercises. The workpeople return from work about the later hour, and have their evening meal afterwards. If the bath has been taken in the morning, there is no need for another then; but if not, and if there is leisure for for it, the addition of a bath to the afternoon exercises will add to their refreshing influences, and to the feeling of ease, lightness, and freshness which they induce.

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Siteism, or *Sitia*. The taking of food (from σῖτος = wheat or corn; used, of bread; hence monositeism or monositia, eating once a day from μόνος; monophageism or monophagia is used of eating alone. some persons being both monositeous and monophageous; hence dissiteism or dissitia, trissiteism or trissitia, tetrasiteism or tetrasitia, pentasiteism or pentasitia, hectasiteism or hectasitia, octositeism or octositia, and decasiteism or decasitia, for eating two, three, four, five, six, eight or ten times a day; pollaki-siteism or pollaki-sitia, and poly-siteism or poly-sitia, the act of eating too often, πολλάκισ, or too much, πολὺς; oligaki-siteism or oligaki-sitia, and oligo-siteism or oligo-sitia, the act of eating too seldom, ὀλιγάκισ = seldom, or only now and then; and too little, ὀλίγος = little or small).

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ERRATA.

P. 80. Line 14. For *pattern*, read *father*.

P. 89. Line 11 from bottom. For *understood*, read *misunderstood*.

P. 182. Line 9. For *exised*, read *excised*.

P. 188. Line 11 from bottom. For *hypethoply*, read *hypertrophy*

The scholar will also please to excuse one or two errors in accents on Greek words, which it proved too late to correct, and to which no further reference is made.

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